Reciprocity and Prejudice:

An Experiment of Hindu-Muslim Cooperation in the Slums of Mumbai

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ABSTRACT

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The dissertation develops and tests a new theory to explain intergroup cooperation and outgroup discrimination. The theoretical part specifies under what conditions ethnic differences undermine public goods provision and exacerbate ethnic discrimination. It posits that people cooperate more with and discriminate less against the groups expected to reciprocate cooperative behavior. Conditional cooperators rationally update their group stereotypes based on their experience with the groups' individual members. This change in turn reduces prejudice and discrimination. I tested observable implications of the model on a representative sample of more than 400 slum-dwellers in Mumbai. The field research in India combined laboratory experiments, an original survey, and interviews. Once I manipulated expectations of reciprocity, ethnically heterogeneous groups produced as much public goods as the homogeneous ones. The experimental treatment also radically increased trust and reduced ethnic discrimination of the generally mistrusted Muslim minority. The survey analysis compared the real-life effect of reciprocity with prominent alternative explanations from the literature. Compared to other factors, positive reciprocity provides a powerful explanation of why people choose to discriminate against some, but not other ethnic groups. The cross-national chapter of the dissertation extends the analysis beyond India. Using surveys from 87 countries, it shows that generalized trust moderates the negative effect of ethnic diversity on people's willingness to contribute to public goods.

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Chapter 1. Rational Model of Intergroup Cooperation and Conflict

1.1 Introduction

On the night of January 8, 1993, a Hindu family of six was sleeping in a simple room at Radhabai Chawl, in a Muslim slum in northern Bombay. In the middle of night, someone blocked their door and threw a petrol bomb in through the window. The whole family perished in the blazing fire that would soon spread out and turn the cosmopolitan metropolis into an inferno. One of the victims was a handicapped teenage girl. Stirred by an unfounded rumor that the handicapped Hindu girl was gang-raped by the same Muslim men who later set her and her parents on fire, a Hindu mob assembled and started to massacre Muslims across the city. A New York-based writer Suketu Mehta interviewed some of the rioters for his Pulitzer finalist book *Maximum City: Bombay Lost and Found*. This is how Sunil, a Hindu rioter, remembers one of their gruesome murders (Mehta 2004, 39):

> “We met a pavwallah on the highway, on a bicycle. I knew him; he used to sell me bread every day. I set him on fire. We poured petrol on him and set him on fire. All I thought was, This is a Muslim. He was shaking. He was crying, 'I have children, I have children!' I said , 'When your Muslims were killing the Radhabai Chawl people, did you think of your children?'”

Sunil's story of killing an acquaintance because of a collective identity seems to support the standard view of ethnic conflict as a principally inescapable phenomenon. Especially in the 1990s, this belief justified passivity of politicians in dealing with ethnic

conflicts in former Yugoslavia, Rwanda, and elsewhere. For example, former President Bill Clinton said about the crisis in the Balkans: “It’s tragic, it’s terrible, but their enmities go back five hundred years, some would say almost a thousand years.” However, despite making headlines by its extreme violence, ethnic conflict is in fact quite rare. One noteworthy analysis of ethnic violence in Africa concluded that only one for every two thousand pairs of ethnic groups with a potential of being in conflict actually fights (Fearon and Laitin 1996). Even when a conflict occurs, not everyone participates in it. While Sunil was setting a Muslim bread seller ablaze, other Hindus protected their Muslim friends and neighbors, very often at the risk of their own lives. An important question is also why the conflict opposed Marathi-speaking Hindus and Muslims, and not for example Hindus vs. Zaroastrian Parsis, or Marathi-speakers vs. Gujarati-speakers, or any other pair of ethnic groups inhabiting this populous city.

Some of the leading theories in social sciences overpredict conflict between groups and often underestimate the possibility of cooperation between strangers. Evolutionary biologists have originally have gone so far as to predict pro-social behavior only among blood relatives. According to the popular gene-centric approach, an individual engages in an altruistic behavior only if it ultimately helps other individuals with the same genes to procreate (Dawkins 1976). As the founding father of population genetics, J. B. S. Haldane, once famously said: “I will jump into the river to save two brothers or eight cousins” (Nowak and Sigmund 2007, 7). This intuition was later formalized in Hamilton's rule: Cooperation is favored by natural selection if the coefficient of genetic relatedness is higher than the cost/benefit ratio of the altruistic act (Hamilton 1996).

Social psychologists acknowledge that solidarity is not necessarily limited by kinship. Instead, favoritism can be extended to culturally defined, yet still bounded units. Social identity theory (Turner 1975; Tajfel and Turner 1986) predicts that an individual should favor her fellow ingroup members over outgroup members even if groups have been created in an arbitrary way – for example by flipping a coin. Positive evaluation of the ingroup raises the member's self-esteem and in turn maintains her identification with the group. Ingroup favoritism can manifest itself in the form of behavior (discrimination), attitudes (prejudice), or pure cognition (stereotype). More often than not, strong group attachment leads to political intolerance (Gibson and Gouws 2000), such as support for anti-immigration policies (Sniderman et al. 2004). According to this view, ethnic conflict naturally arises from incessant intergroup comparison and groups' concern with their position relative to other groups rather than with absolute well-being (Horowitz 1985).

Social identity theory shares its belief in universal ingroup favoritism with primordialism – a historically influential but nowadays almost abandoned theory of ethnic politics. Primordialism claims that ethnic affinity is deeply rooted in history (Smith 1987) and essentially unchangeable by rational calculus or social interactions (Geertz 1973). Nations, such as Croats and Serbs, are consequently locked in a perennial conflict fueled by ancient hatred (Kaplan 1993). However, a more careful reading of history reveals that supposedly ancient hatreds and even national identities themselves are much more recent and fluid (Eller and Coughlan 1993; Fearon and Laitin 2000). Furthermore, primordialism can hardly explain considerable variation of intergroup conflict in time and space. The same groups sometimes fight each other in some places, while living peacefully in others. To use a classic example from the political science

literature, the very same tribes of Chewas and Tumbukas are political allies in Zambia and opponents in the neighboring Malawi (Posner 2004). Patterns of intergroup conflict and cooperation often vary even within the same polity. The bloody ethnic riots in 1992-93 swept through the slums of the Indian city of Surat, while sparing Surat's old town (Varshney 2002).

Whereas social identity theory traces intergroup conflict to social categorization itself, realistic group conflict theory (Sherif et al. 1961) adds another condition: presence of a limited resource. According to realistic group conflict theory, hostility between groups arises when they compete for scarce resources and their interests are incompatible. The famous Robbers Cave Experiment, described in Sherif et al. (1961), demonstrated how vicious competition between two groups of 11 year-old boys desiring a summer camp trophy can become. In the realm of politics, conflicts usually occur over the scarce resources of patronage (Posner 2005) or high social status (Petersen 2002). In Posner’s account, ethno-political cleavages arise between groups large enough to form a minimum wining coalition capable of seizing scarce resources of patronage and political power in a country. Instead of political conflict being fueled by salient ethnic identities, electoral rules and other institutions determine which ethnic groups are big enough to become salient social categories. Petersen describes patterns of ethnic violence in western Ukraine during World War II. After this previously Polish territory was occupied by the Soviets in 1939, the low-status Ukrainian population gave way to their resentment of the former Polish elite and attacked Polish landowners and military colonists. Ethnic violence is portrayed here as emerging when a sudden change in power makes a previously underprivileged group believe that they can subordinate a high-status group. In addition

to power and status, hostility to minorities is sometimes driven also by economic concerns of the dominant group (Quillian 1995; Sniderman et al. 2004).

This dissertation proposes a new explanation of ethnic conflict and cooperation. It posits that people cooperate more with and, consequently, discriminate less against the groups expected to reciprocate cooperative behavior. Conditional cooperators rationally update their group stereotypes after experiencing reciprocity in interactions with the groups' individual members. This change in turn reduces prejudice and discrimination. Chapter 1 outlines the new theory and derives its observable implications. Chapter 2 describes the environment of Indian slums, in which the theory was tested experimentally. Chapter 3 focuses on the experiments themselves. Increasing expectations of cooperative behavior in a laboratory increased trust and reduced ethnic discrimination of a generally mistrusted ethnic minority.

Chapters 4 and 5 then use the theory to explain two important puzzles from the literature on ethnicity. Chapter 4 addresses the puzzling observation that the same person discriminates against some but not all outgroups. For example, Black Africans' ethnic group identification in South Africa is associated with negative attitudes towards Afrikaans Whites, but not towards English Whites (Duckitt and Mphuthing 1998). Much of the current research in social psychology analyzes individual attributes, such as self-esteem (Tajfel and Turner 1986), authoritarian personality (Adorno 1950), or social dominance orientation (Sidanius 1993), in search for a universal explanation of intergroup conflict. However, these explanations are generally not consistent with the fact that people distinguish between different outgroups. Although an authoritarian personality may explain why someone has a higher propensity for discrimination in

general, it can tell us little about why the same person would discriminate against Afrikaans Whites but not against English Whites.

Several scholars have used group attributes instead to explain why only few groups compete against each other. According to Blalock (1967), the group's size is what matters. A greater relative size of the minority group to the dominant group would threaten the latter because of increased competition for scarce resources and the minority's higher potential for political mobilization. In his book on electoral politics in Zambia, Posner (2005) argued that ethno-political cleavages appear between groups, whose size allows them to be part of a minimum winning coalition. Quillian (1995) found a positive association between the proportion of immigrants and strength of racial prejudices in Western Europe. However, a cross-country study of intrastate conflict by Ellingsen (2000) did not find a statistically significant effect of a large minority. Moreover, the relative group size argument cannot explain why discrimination often targets small or disenfranchised minorities. For example, Haitians represent only three percent of the population of the Dominican Republic, Whites less than one percent of the population of Zimbabwe, and Bahá'ís less than 0.5 percent of the population of Iran. Comparably, governments in Indian states tend to protect the Muslim minority against ethnic riots if it is large enough to give them votes (Wilkinson 2004). Reciprocity helps explain this puzzle in Chapter 4.

Chapter 5 applies the theory on the field of economic development. The "natural" preference for one's group presumed by the literature has substantial implications for economic development of multiethnic countries. Much empirical research indicates that ethnic diversity leads to suboptimal public goods provision and hinders development.

Several reasons have been proposed. Divergent policy preferences of racial groups living in the same country can lead to a disagreement about which public goods should be provided (Alesina et al. 1999; Kinder and Winter 2001). Alternatively, production of public goods in homogenous societies can be facilitated by more effective social sanctioning – an advantage that ethnically heterogeneous countries lack. As Habyarimana et al. (2009, 104) concluded in their experimental study, "ethnically homogeneous communities may have an advantage in providing public goods because those individuals least likely to contribute are also particularly sensitive to threats of retaliation should it be known that they have failed to contribute to collective endeavors." One possible mechanism behind this effect is the simple observation that it is easier to find and punish someone who exploited one's trust if the person belongs to one's own group (Habyarimana et al. 2007, 2009). Bad reputation of an untrustworthy person can also spread through social networks. Since social networks are usually denser within than across ethnic groups, it should be easier to obtain information about the past of a coethnic (Fearon and Laitin 1996).

In the 1990s, development economists did indeed pinpoint ethnic fragmentation as a cause of low schooling and inadequate infrastructure in Africa, the most underdeveloped region of the world (Easterly and Levine 1997). For example, Miguel and Gugerty (2005) found that Kenyan communities with an average ethnic diversity generated 20 percent less contributions for their schools than ethnically homogenous communities. Five thousand miles away, households in mixed communities in Indonesia are less likely to contribute money and labor to local health centers, rice cooperatives, neighborhood irrigation associations, and other voluntary organizations (Okten and Osili

2004). Investment in public goods, from education to roads to trash pickup, is inversely related to racial diversity also in U. S. cities (Alesina et al. 1999). In addition to insufficient public goods provision, ethnically diverse communities in the United States are generally characterized by lower levels of cooperation, altruism, and trust (Putnam 2007). For a larger sample of countries, Alesina et al. (2001) found a negative correlation between ethnic fractionalization and the government's social spending. Due to all the empirical evidence, "the notion that social divisions undermine economic progress" has become "one of the most powerful hypotheses in political economy" (Banerjee et al. 2005, 639).

However, the effect of ethnic diversity is not uniformly malign. Miguel (2004) compared two nearby and similar districts, separated only by the Kenyan-Tanzanian border. Kenyan communities at an average level of ethnic diversity raised 25 percent less funding for their schools than homogenous communities. Across the border, in Tanzania, heterogeneous communities were equally successful as their homogenous counterparts. In the United States, racial fractionalization is associated with poor public policies, while other measures of cultural diversity seem to rather correlate with higher productivity (Alesina and La Ferrara 2005). When we look at developed countries with a high number of immigrants, such as Canada or New Zealand, they are actually doing quite well compared to the rest of the world. If there is any general conclusion, it is that "similar levels of ethnic diversity are associated with very different degrees of conflict and interethnic cooperation" (Alesina and La Ferrara 2005, 794). Understanding the specific conditions under which ethnic diversity decreases cooperation and public goods provision is not only an interesting theoretical question, but also an urgent political issue because of

the growing numbers of immigrants in most societies (Putnam 2007). Chapter 5 of this dissertation offers an explanation of why ethnic diversity reduces people's willingness to contribute to public good in some but not other contexts.

1.2 Argument

1.2.1 Reciprocity

The idea that reciprocity sustains cooperation is not new. As early as 1906, social scientists hailed reciprocity as "the vital principle of the society" (Hobhouse 1906, 12). Almost a century of research later, a *Science* article reiterated this view: "Reciprocation is the basis of human cooperation" (Nowak and Sigmund 2000, 819). Gouldner (1960) suggested that the moral norm of reciprocity is one of the universal social norms, present in most if not all moral codes. Early research on the iterated prisoner's dilemma game showed that the most successful strategies are those of conditional cooperators. The particularly successful "tit for tat" strategy consists of always cooperating in the first round and then reciprocating what the other player did on the previous move (Axelrod 1984). As conditional cooperators reinforce each other, this strategy can be viable even if everyone else in the population defects (Axelrod and Hamilton 1981). Evolutionary models show that a population of indiscriminate altruists will be quickly invaded by free riders (Bowles and Gintis 2004). Only if a sufficient number of conditional cooperators refuse to cooperate with free riders can cooperation be sustained (Nowak and Sigmund 2005). Such discrimination between likely cooperators and defectors is regarded as the

society's "immune response" against free-riding (Nowak and Sigmund 2000, 819).

Trivers (1971) concluded that animals have evolved to respond in kind to the actions of others. Therefore, he put what he called "human reciprocal altruism" into the same category as alliances among baboons and warning calls in birds. On the other hand, Hill et al. (2011) argued that extensive cooperation between genetically unrelated individuals differentiates humans from other animals. Whatever the truth is, experimental data from around the world support the view that humans evolved to be *conditional cooperators*; they typically reward cooperation and punish defection (Ostrom 2000; Fischbacher et al. 2001; Chaudhuri 2011).

It is easy to punish defection in repeated face-to-face interactions with the same partner. However, much of the modern world relies on anonymous transactions with people we do not know: customers buy goods from complete strangers at eBay (Resnick et al. 2000), political parties buy votes despite their essential inability to monitor the secret ballot (Brusco et al. 2004), and even personal security can be threatened by people we have never met. Something as routine as paying taxes is akin to cooperation with a very large number of anonymous strangers. Moreover, many transactions are not instantaneous, but rather involve delayed payment. For example, online shopping customers pay with expectation that the website's owner will deliver the goods, banks lend money with expectation that the loan will be repaid, and voters hope that their elected representative will keep at least some of her promises.

To make the society's immune response against free riders work outside the scope of repeated face-to-face interaction, conditional cooperators have to rely on their potential partner's reputation (Nowak and Sigmund 2007). But if the reputation of another person

is not readily available, what information can we use to identify likely cooperators and free riders? I argue that humans use *group membership* as an information shortcut. If group membership is the only information available, the individual *i* estimates whether the individual *j* will cooperate or defect by looking at the record (real or imagined) of cooperation and defection of other members of *j*'s group. This logic should be even more important if we are interested rather in an aggregate of individuals (e.g. large groups contributing to a public good). I define the expectations of cooperative behavior as the probability perceived by the individual *i* that the individual *j* will cooperate with *i*. While *i* can estimate *j*'s general propensity to cooperate from *i*'s stereotypical belief's about *j*'s group, the expectation of interest also depends on *i*'s attributes. For example, a neo-Nazi *i* may not expect any help from a Jewish person *j* despite her strong belief that *j* would go to great pains to support other Jews.

1.2.2 Stereotypes, Prejudice, and Discrimination

It is a normal cognitive function to classify people into social categories based on visible characteristics, such as race or gender, and to view thusly formed groups as homogenous in terms of personality traits (Fiske 2000; Yzerbyt and Demoulin 2010). For example, Blacks in the American society are often seen as "lazy" and "criminal" (Devine and Elliot 1995). Although objectionable on moral grounds, stereotyping itself is a banal cognitive strategy that the human mind uses to reduce the complexity of the social world by transforming innumerable fuzzy differences between individuals into few clear dichotomies. As many other behavioral characteristics have become subject to group

stereotypes, there is no reason to believe that general expectations of cooperative behavior are immune to this natural tendency.

Following Weber and Crocker (1983, 961), I define stereotypes as beliefs about the characteristics of a specific group. I further define *group* as any social category in which an individual may be a member. This definition is very similar to Kanchan Chandra's definition of "identity" (Chandra 2006). Social categories are sets of people given a common label and distinguished by (1) rules of membership that decide who is and is not a member of the category, and (2) sets of characteristics (such as beliefs, physical attributes, or behavior) thought to be typical of members of the category.[1] Men, the youth, and university students are all examples of groups. Each membership in a group is represented in the member's mind as a s*ocial identity*.

Group is a much broader category than social group, whose definition typically also includes common goals and interaction between members. Accordingly, my definitions of *ingroup* and *outgroup* are also broader than their usual definitions in sociology and social psychology. In this dissertation, ingroup is a group, in which an individual is eligible to be a member, and outgroup is a group, in which an individual is not eligible to be a member. This means that although a social category of "white people" may be too large and distant to be a true social group (and therefore an ingroup) in the eyes of most social psychologists, this dissertation would consider it a meaningful ingroup. Another conceptual difference is the distinction between ingroups and outgroups. Followers of social identity theory define ingroup by the individual's feeling of loyalty or belonging. They define outgroup by contempt or antagonism. Since my

[1] This definition closely follows Fearon and Laitin (2000).

dissertation studies ingroup bias as a dependent variable, this definition would be circular and I opted for a more neutral definition by membership eligibility instead.

Although the proposed model is general enough to be applicable to any groups, the rest of the dissertation focuses on ethnic groups because they are most relevant for political scientists and policy makers. Following Chandra (2006), I define *ethnic group* as a group, in which eligibility for membership is determined by attributes associated with, or believed to be associated with, descent. Examples of descent-based attributes include skin color, language, last name, and ancestral religion.

Many different triggers can activate stereotypes about groups and lead consequently to *prejudice* (Devine 1989; Wheeler and Petty 2001). Similarly to Allport (1954), I define prejudice as antipathy (1) based on group-level generalization and (2) directed either toward a group as a whole or toward an individual because of her group membership. There is empirical evidence that beliefs about cooperative behavior translate into sympathy and antipathy. Successful social exchange with another person increases liking of that person and produces positive feelings of gratitude and pleasure (Lawler 2001). Free-riding, on the other hand, causes strong emotions of anger and sadness (Fehr and Gächter 2000; Lawler 2001). The more the free-riders deviate from the group's expectations, the stronger these emotions are (Fehr and Gächter 2000).

If one’s group supports or fails to police discriminatory behavior, prejudices may manifest themselves in the form of *discrimination*, ranging from selective hiring to hate crimes (Yzerbyt and Demoulin 2010). If I believe that all immigrants are crooks, I will be less likely to offer an immigrant a job, lend her money, or enter into any transactions based on trust with her. As discrimination prevents a person from obtaining resources by

cooperation, competition between groups becomes paramount. Although prejudices do not always lead to overt discrimination, empirically, there is a positive correlation between the two (Schütz and Six 1996), as well as between stereotypes and discrimination (Cuddy et al. 2007). Discrimination in this work is understood broadly as differential treatment of individuals based on their group membership, such as ethnic affiliation (Fershtman and Gneezy 2001).

Stereotypes and prejudices may be based on real information (e.g., the group's social norms) or their basis can be entirely faulty. According to theory of symbolic politics, people develop a limited set of highly stable symbolic predispositions, including partisanship, ideology, and ethnic prejudices, early in their lives (Sears and Funk 1999). Since so much socialization occurs in families, stereotypes can be easily inherited from previous generations. Some psychologists see prejudice as a manifestation of deep-rooted personality traits. For example, Adorno (1950) used his interviews with former Nazis to argue that people with an authoritarian personality suppress the groups over which they have authority. Sidanius (1993) asserted that prejudice correlates with an individual's acceptance of social hierarchies, captured by the social dominance orientation scale.

According to another longstanding argument, stereotypes are formed by mass media, though empirical evidence is mixed (Green and Seher 2003). In a now-classic study, Wade (1987) highlighted a nefarious impact of the 1915 epic film *Birth of a Nation*, which glorified the then waning Ku Klux Klan and depicted African American men essentially as sexually aggressive brutes. More recent behavioral studies have demonstrated that political propaganda can make certain prejudices more or less salient. For instance, Gilliam and Iyengar (2000) found that the local television's stereotypical

portrayal of crime perpetrators as non-white males heightened whites' negative attitudes against blacks. Although prejudices are sometimes evoked during electoral campaigns, politicians in the United States are no longer able to play race card too overtly (Mendelberg 2001). Moreover, there is little evidence for a more far-reaching idea that narratives in mass media can *create* new attitudes (Green and Seher 2003). Even the effect on salience is not guaranteed. Hovland et al. (1949) demonstrated that the U.S. Army's series of propaganda films entitled *Why We Fight* did in fact little to increase soldiers' patriotic attitudes. Although this dissertation does not study where prior beliefs about cooperation come from, it is likely that they are formed just like other stereotypes by a combined effect of family, schooling, media, and personal experience.

1.2.3 Intergroup Contact

Whatever their origins are, prejudices sometimes change in a dramatic way. For example, Schuman et al. (1985) described how the American public opinion moved away from the belief that whites should have a priority over blacks in the public life between the 1940s and 1970s. One particularly influential explanation of why prejudices change is the *contact hypothesis*. More than a half-century ago, Gordon Allport proposed that under specific conditions, intergroup contact reduces prejudice. Since then, the contact hypothesis has influenced generations of scientists and policy-makers. Racial integration in the U.S. Army during World War II provides an illuminating example of benefits derived from intergroup contact. According to Putnam (2007), 62 percent of white soldiers with no contact with black troops opposed mixing black and white platoons in

their company. In the regiments with some black platoons, this number dropped to 20 percent. Among white soldiers, whose companies have already been mixed, only 7 percent opposed the policy. As Pettigrew and Tropp (2006, 751) unequivocally concluded in their widely cited survey of contact-hypothesis studies, “with 713 independent samples from 515 studies, the meta-analysis finds that intergroup contact typically reduces intergroup prejudice.” In fact, 94% of the samples included in their meta-analysis exhibited an inverse relationship between contact and prejudice.

That said, not all contact between groups is supposed to be beneficial. Pettigrew (1971, 275) summarized the optimal conditions of a positive contact as such: “Prejudice is lessened when the two groups (1) possess equal status, (2) seek common goals, (3) are cooperatively dependent upon each other, and (4) interact with the positive support of authorities, laws, or customs.”

As a set of restrictive conditions like this is rarely achievable in practice, few researchers have managed to replicate the optimal conditions and measure their effect. An example of a successful test of the contact hypothesis under the most favorable conditions is a field experiment by Green and Wong (2009). 54 white teenagers participated in wilderness courses. They were randomly assigned to either an all-white or a racially mixed group. All members had an equal status, shared the same goal (survival in wilderness), cooperatively depended on each other, and their interaction was supported by a qualified group leader. One month after completing the course, members of racially heterogeneous groups reported higher levels of tolerance.

It is unclear whether the optimal conditions of positive contact are jointly or independently sufficient. Pettigrew and Tropp (2006, 751) argued that they “are best

conceptualized as an interrelated bundle rather than as independent factors." Unfortunately, bundling four very different conditions together in empirical studies makes it difficult to discern their individual effects. In their meta-analysis, Pettigrew and Tropp (2006) found evidence suggesting that the optimal conditions were not necessary, but the positive support of authorities did significantly increase effectiveness of contact.

Allport (1954, 268) merely hinted at the possible causal mechanism behind his theory: "Contacts that bring knowledge and acquaintance are likely to engender sounder beliefs concerning minority groups, and for this reason contribute to the reduction of prejudice." This dissertation argues that it is not simply *any* information updating that matters. What matters is specifically increasing expectations of cooperative behavior. In line with the logic of reciprocity, experiencing cooperative behavior during intergroup contact can change the stereotypical beliefs – and by extension also prejudices – about the outgroup.

I hypothesize that observing individual behavior can change stereotypes at the group level. This proposition is not without an empirical basis. According to an experimental study by Weber and Crocker (1983), people respond to information that deviates from their stereotypes by updating beliefs. Every new piece of evidence causes a minor change in the stereotype. Updated beliefs also affect attitudes and behavior. Peffley et al. (1997) found that information contradicting pre-existing racial stereotypes changed discriminatory political attitudes even among people with a strong racial bias. What kind of evidence do we need to change stereotypes about propensity to cooperate?

Affect theory of social exchange predicates that a person-to-group tie is more salient in *indirect exchange* and positive emotions created by a successful exchange are

attributed to the group as a whole (Lawler 2001).[2] This contrasts with direct exchange, where a person-to-person tie is more salient (Lawler 2001). Molm et al. (2003) found that indirect exchange increases perceptions of fairness. On the other hand, negotiated exchange actually makes competition more salient and triggers unfavorable perceptions of the other side as unfair (Molm et al. 2003). Molm's finding may help explain why intergroup contact in work situations usually does not lower prejudice (Amir 1969). Drawing on this line of research in psychology, I focus on *indirect positive reciprocity*, which can be defined in the words of Nowak and Sigmund (2005, 1291) as "I help you and somebody else helps me."

1.3 Predictions

Table 1: Predicted Group Bias Depending on Expected Cooperative Behavior

		Outgroup	
		Cooperates	Defects
Ingroup	Cooperates	No bias	Ingroup bias
	Defects	Outgroup bias	No bias

Table 1 summarizes testable implications for discrimination depending on the cooperative behavior from expected from ingroup and outgroup members. The first cell of the table corresponds to the case of generalized reciprocity. People expect others to

[2] Indirect exchange arises when a third party uninvolved in the initial interaction returns the favor sometime in the future (Befu 1977). Negotiated exchange "incorporates collective decision-making, advance knowledge of terms, mutual assent, and binding agreements" – think of trade and employment (Molm et al. 2003, 128).

cooperate regardless of group membership. There should be no discrimination between the two groups.

The model predicts ingroup bias only in one special case – if ingroup members are expected to cooperate, while outgroup members are not (second cell). This implication significantly departs from social identity theory (Tajfel and Turner 1986; Turner 1975), according to which a person should always favor her fellow ingroup members over outgroup members.

The third cell of Table 1 predicts outgroup bias, with a rational individual preferring the cooperative outgroup over the untrustworthy ingroup. Although counter-intuitive, outgroup bias can be sometimes observed empirically. For example, a Eurobarometer survey in 1996 asked citizens of 15 member states of the EU whether they trusted people from various European and non-European countries. Surprisingly, Italians trusted the Swiss, Swedes, and Americans more than their own countrymen. Swedish respondents had more trust in Norwegians than in other Swedes. Similarly, Chinese participants in an economic experiment described by Buchan et al. (2006) found outgroup members more trustworthy than ingroup members during an investment game.

The fourth cell describes a situation of general mistrust and is thus the polar opposite of generalized reciprocity. Whereas the default strategy in the first cell was to cooperate with everyone, here it is rational to defect with everyone. Of course, cooperation is possible even in the fourth case if there is an alternative way to assure reciprocity. For example, a more credible threat of punishment of cheaters if they belong to the same community as the victim may transform the fourth cell to the second cell.

These four equilibria are stabilized by an inherent reinforcement mechanism.

Cooperation reduces discrimination, which creates even more opportunities to cooperate with the same group, while preexisting mistrust prevents future cooperation that could otherwise help overcome it. The causal mechanism described in this dissertation may create virtuous cycles of cooperation or vicious cycles of conflict. A slightly higher cooperation rate in a well-bounded group for example due to more effective enforcement increases expectations of future cooperation, which convinces more members to cooperate, which leads to more positive reciprocity… Similarly, a member of a stigmatized group may want to cooperate, but ultimately defects because she does not believe that a high-status person would trust her enough to reciprocate cooperation. Of course, such preemptive defection will only reinforce the high-status group's stereotypes, creating a negative feedback loop of mistrust and deteriorating intergroup relations.

If preferences regarding cooperation with specific groups are common knowledge, the four strategies will become focal points used by both groups to coordinate their behavior. In this way, patterns of discrimination can be rectified for a long time, until an exogenous shock breaks the vicious cycle of mistrust. Varshney (2002) provided an example of virtuous and vicious cycles predicted by this study in his book on ethnic riots in India: 90 percent of Hindu and Muslim families in the peaceful city of Calicut, but only 42 percent in conflict-prone Aligarh, reported that their children played together. Aligarhs of our world may never improve without introducing more opportunities for interethnic cooperation. On the other hand, once reciprocal cooperation becomes institutionalized, it can protect the community from ethnic violence for centuries – as Jha (2008) demonstrated in the case of Hindu-Muslim relations in former medieval trading ports in India. Chapter 6 will discuss ways, in which we can increase expectations of

reciprocity in multiethnic societies.

For cases with a prior ingroup bias, the causal argument of the paper implies that an exogenous increase in an outgroup's indirect positive reciprocity should reduce discrimination against the said outgroup (first column of the Table 1). This is exactly what a public goods experiment described in Chapter 3 achieves by manipulating cooperative behavior across treatment groups. The process can be envisaged as Bayesian updating of expectations of cooperative behavior. A person has some priors based on groups stereotypes that she holds. As I explained above, negative stereotypes may lead to prejudice and discrimination. When this person encounters a member of the outgroup *and* the contact results in a cooperative interaction, stereotypes get updated. More positive posterior beliefs reduce the level of prejudice and discrimination.

The simplified dyadic image sketched out in the previous section can be extended easily to situations with many different outgroups. When faced with various groups, a person should favor those whom she trusts to reciprocate cooperative behavior. Consequently, discrimination should negatively correlate with the expectations of cooperative behavior. Chapter 4 observes this hypothesized effect of reciprocity in survey data. It also compares the effect of reciprocity to the most prominent alternative explanations of discrimination in the literature on ethnic politics.

In addition to explaining selective discrimination, the model has an important implication for economic development at the societal level. In the societies ruled by the social norm of generalized reciprocity (first cell of Table 1), ethnic identity should not be an obstacle to cooperation. If the lack of interethnic cooperation hinders economic development – as is assumed by much of the literature in the field of development

economics – ethnic diversity of the country should correlate negatively with economic growth and public goods provision only in the countries without the generalized expectations of reciprocity. Ethnic diversity should not affect people's willingness to contribute to public goods in the societies characterized by generalized reciprocity. Chapter 5 tests this hypothesis using cross-national survey data.

1.4 Structure, Agency, and Social Construction of Identities

This essentially rational model specifies structural conditions of discrimination. It does not compete with – it complements – agency theories explaining when discrimination escalates into open ethnic conflict. For example, Gagnon (1995) and Des Forges (1999) highlighted the role of elite manipulation in instigating the Yugoslav wars and the genocide in Rwanda respectively. Whereas my argument may help elucidate why Slobodan Milošević chose to demonize Kosovar Albanians and not Vojvodina Hungarians, it does not cleanse him from the guilt of mobilizing masses of his supporters by ethnic hatred. As is often the case in politics, structure and agency interact. Prior beliefs constrain political elites in their hostility against a cooperative outgroup; preexisting mistrust paints a bull's eye for an ambitious ethnic entrepreneur to aim her rhetoric. Negative stereotypes may also help us understand better instances of ethnic massacres without leadership, such as those discussed in Petersen (2002). Conversely, encouraging positive reciprocity in post-conflict societies may reduce prejudice and distrust between groups, whose identities have been previously constructed as antagonistic by political elites. Despite having important implications for the study of

ethnic conflict, this dissertation focuses only on one causal mechanism in a long causal chain leading to ethnic violence.

The proposed causal mechanism interacts with other structural conditions, such as market integration. Although the relationship between stereotypes and discriminatory behavior is mediated by emotions (Cuddy et al. 2007), the direct link between the two is probably of a more rational nature. When Cicero proclaimed "there is no duty more indispensable than that of returning a kindness," he quickly added an instrumental reason: "all men distrust one forgetful of a benefit" (Gouldner 1960, 161). People do not care very much about others' propensity to cooperate if they have nothing to gain from cooperation or if it is easier to obtain resources by coercion (Gouldner 1960). For example, Hutus and Tutsis at the time of the genocide in Rwanda were not in complementary roles: "Tutsis raise cattle primarily for their own use, while Hutus raise crops primarily for their own consumption, and both Rwanda and Burundi are organized primarily in agricultural subsistence units that spread over the land" (Hardin 1995, 170). Contrast Rwanda with the Indian city of Lucknow, in which economic interdependence between Hindu traders and Sunni Muslim workers arguably prevented serious ethnic rioting that have plagued other Indian cities in the 1990s (Varshney 2002). Death or flight of skilled workers during the riots would cause a collapse of the local economy and was thus against the interest of Hindu leaders regardless of what they thought about the Hindu nationalist cause. As a large cross-cultural study of small-scale societies all around the globe concluded more generally, "the higher the degree of market integration and the higher the payoffs to cooperation, the greater the level of cooperation in experimental games" (Henrich et al. 2001, 74). It is very likely that a sufficient level of market

integration is necessary for a beneficial effect of positive reciprocity to take place.

Finally, the theory is about relations between groups that already exist as salient social categories. Different theories explain how groups with distinct social identities emerge at the first place. For example, Weber (1976) described how compulsory education and conscription contributed to nation-building in France, Anderson (1991) attributed the birth of modern European nations to “print-capitalism”, and Marx (1998) studied the black identity as a function of exclusionary policies instituted by the majority. Acknowledging contributions of this strand of literature, the dissertation focuses on social identities that have already become salient.

Two prominent schools in social psychology, identity theory and social identity theory, concur that an individual holds multiple identities at the same time (Hogg et al. 1995). Famous experiments by Henri Tajfel showed that new collective identities can be created literally in minutes, and based on trivial characteristics, such as preference for Kandinsky or Klee (Tajfel et al. 1971). The dominant constructivist paradigm in the field of ethnic politics also adopted a view of social identities as multiple and malleable. Eric Hobsbawm once summarized this notion in a cogent example: “Whether a Mr. Patel in London will think of himself primarily as an Indian, a British citizen, a Hindu, a Gujarati-speaker, an ex-colonist from Kenya, a member of a specific caste or kin-group, or in some other capacity depends on whether he faces an immigration officer, a Pakistani, a Sikh or Moslem, a Bengali-speaker, and so on” (Hobsbawm 1996, 1067). Theories other than mine explain Mr. Patel’s choice of a particular identity from his identity repertoire. People may identify with a high-status group (Laitin 1998) or a group strong enough to win elections (Posner 2005), for instance. On other occasions, individuals are not free to

choose their social identity. Jews during the Holocaust and Tutsis during the Genocide in Rwanda died with an identity ascribed to them by their murderers. This dissertation does not explain individual identity choice, but attitudes towards other groups once the ingroup identity is fixed in the short term. Expanding on Hobsbawm's example, my model does not explain why Mr. Patel would emphasize his Gujarati identity over his British identity, but it may explain why he, as a Gujarati, would do business with a Parsi and not with a Bihari.

Chapter 2. Ethnic Conflict and Cooperation in Slums

Following Harry Eckstein's case-selection criteria, I chose the *least-likely crucial case* to test the proposed argument (Eckstein 1975) in Chapters 3 and 4. Chapter 5 then extends the analysis to the global level. A least-likely case is one that "on all dimensions except the dimension of theoretical interest, is predicted not to achieve a certain outcome, and yet does so" (Gerring 2007, 115). As I explain below, I found such a tough test in the slums of Mumbai, India.

2.1 History of the Hindu-Muslim Conflict in Mumbai

Communal violence between Hindus and Muslims has been a major issue in Indian politics for a long time. But even in the Indian context, Mumbai gained notoriety as the country's historically most riot-prone city, with 1,137 recorded deaths in 1950-95 (Varshney 2002). History of ethnic violence in Mumbai – previously known as Bombay – goes back for at least more than a century. After some small-scale clashes in the second half of the 19th century, the first serious ethnic riot in Bombay took place in 1893, killing 80 people and wounding 530 (Kidambi 2007). Anticipating similar spells of violence in the 20th century, a local newspaper described "the destruction of temples and mosques, and the looting of shops, to the accompaniment of fierce faction fights in the streets, varied with senseless assaults on the police and the military" (Kidambi 2007, 118). In 1929, low-level violence between Muslim Pathans, infamous as often unscrupulous money lenders, and Hindu textile mill workers escalated into another large-scale Hindu-

Muslim riot (Hansen 2001). An anti-British naval strike spilled into Hindu-Muslim violence resulting in 250 deaths in 1946 (Hansen 2001). The deadly riot in 1984, which claimed 278 lives, was so shocking that it is sometimes seen as the final blow to the perception of Bombay as a cosmopolitan city (Punwani 2005). The latest recorded riot between Muslims and the mostly Hindu police occurred on August 11, 2012 (52 casualties).

The single worst episode of the perennial ethnic conflict in Mumbai followed the destruction of the Babri Mosque in Ayodhya by a mob of Hindu extremists in December 1992. Hindu nationalist parties targeted the dilapidated 16th-century mosque because it was allegedly built on a site, where god Ram, the hero of the national epic Ramayana, was born. The demolition sparked an increasingly violent series of Muslim protests that escalated into full-blown street fighting between Muslim and Hindu mobs. The police intervened, killing disproportionately more Muslims than Hindus (Srikrishna 1998). The second, more serious round of rioting began in January 1993 in a response to the slaughter of a Hindu family at Radhabai Chawl, described at the beginning of the dissertation. Despite much spontaneous violence, the riot was to a great degree orchestrated by the local fascist party Shiv Sena and occurred with complicity of the police (Engineer 1993; Srikrishna 1998; Hansen 2001). According to an extensive official report on the riots, "the attacks on Muslims by the Shiv Sainiks were mounted with military precision, with list of establishments and voter's lists in hand" (Srikrishna 1998).[3] In one conversation taped off the police frequency during the riots, the control center not only refused to prevent arson, but also encouraged the officers on the ground to

[3] Shiv Sainiks are members of the paramilitary wing of the Shiv Sena party.

participate in it. In this incident, a police officer referred to the Muslim owner of the burnt property as to a “circumcised prick” (Mehta 2004, 50).

Two months of rioting left 900 dead, among whom 575 were identified as Muslims and 275 as Hindus (Srikrishna 1998). Many victims were burnt alive and bodies of others were routinely mutilated (Engineer 1993). Rioters razed or burnt down nearly 10,000 houses and an estimated 200,000 people fled the city (Engineer 1993) in a frantic exodus immortalized by the Oscar-winning movie Slumdog Millionaire. The riots of January 1993 alone cost the city an estimated $3.6 billion in property losses, lost production, sales, and taxes (Wilkinson 2004).

The riots also changed the ethnic composition of this once cosmopolitan city. Large numbers of Muslims previously living among the Hindu majority left their scattered enclaves and moved to few overcrowded, but more defensible ghettos (Hansen 2001; Shaban 2010). As an unintended consequence, the local gangs supposedly protecting these Muslim communities from an eventual future riot contribute to the negative stereotyping of Muslims as criminals in the peacetime (Shaban 2010). To the constant ire of Muslims, organizers of the riots, most notably Shiv Sena's former leader Bal Thackeray, have never faced justice. My own field work in Mumbai in 2011 showed that the riots of 1993 are still the single most important item in the city’s collective memory. Every conversation about ethnic relations in Mumbai inevitably drifted to the topic of riots.

Although ethnic riots have become more of a terrifying memory than a serious threat, frequent terrorist attacks attributed to Indian Muslim terrorist organizations with ties to Pakistan regularly rekindle the simmering conflict between Hindus and Muslims

living in Mumbai. The first large-scale terrorist attack came as a response of the Muslim criminal underworld to the 1993 riots. On March 12, 1993, ten powerful bombs exploded at Mumbai's landmarks, including Bombay Stock Exchange. This unprecedented attack killed 251 people and injured 713. Many victims of the riots welcomed the blasts as a justified retaliation.

On December 6, 2002, right on the 10th anniversary of the destruction of the Babri Mosque, a bomb exploded inside a public bus. It killed two people and injured 28. On January 23, 2003, another bomb killed one and injured 28 near the Vile Parle railway station, which, coincidentally, I was using every day during my field work in Mumbai. On March 13, 2003, a bomb in a ladies' compartment of a commuter train killed 10 and injured 70. On July 28, 2003, another blast in a public bus killed four and injured 32. On August 25, 2003, two car bombs went off, killing 54 and injuring 244 people. One of the bombs was detonated at the most popular tourist attraction, the Gateway of India. On July 11, 2006, seven bombs exploded in overcrowded suburban trains over a period of just 11 minutes. The blasts scheduled for a rush hour killed 209 commuters and injured more than 700...

A terrorist attack that mesmerized media worldwide occurred on November 26, 2008. A small band of Pakistani terrorists landed with assistance of an unknown number of locals and attacked multiple locations, including Mumbai's main train station, the famous Taj Mahal Palace Hotel, and a center run by an international Jewish movement. The 60-hour drama, full of indiscriminate shooting, explosions, and hostage murdering, was followed by millions of TV viewers. The final body count reached 172.

The most recent terrorist attack in Mumbai incidentally happened just after I

finished my field research there. In the evening of July 13, 2011, bombs exploded at several crowded locations across the city. They killed 26 and injured 141 people.

I expect that salience of the ethnic violence between Hindus and Muslims will make Mumbaikars less likely to care about reciprocal cooperation.[4] The dissertation also helps increase the miniscule number of experimental prejudice-reduction studies among antagonistic groups (Paluck and Green 2009). At the same time, ethnic polarization in the slums creates a baseline quite different from that in more peaceful societies. Rational updating of stereotypes may have arguably a weaker marginal effect, for example, in Manhattan than in Mumbai because stereotypes themselves may be weaker. Only replication studies in a less biased society can address this issue.

2.2 Population of Interest

India's biggest city would be an interesting case even without an ongoing ethnic conflict. RAND Corporation aptly called Mumbai "India's Wall Street, its Hollywood, its Milan" (Rabasa et al. 2009, 1). The biggest city in India, it is poised to become the world's third biggest urban agglomeration after Tokyo and Mexico City (MCGM 2010). The metropolis has an ethnically very diverse population, with 44 percent being immigrants from other parts of India (MCGM 2010). Among the immigrants, most people came from the rest of the state of Maharashtra (37%), Uttar Pradesh (24%), and

[4] At the time of my field research, antagonism between Hindus and Muslims was also heightened by, of all things, cricket. Not only is cricket the Indian national sport, but the ICC World Cup was played on the Indian soil, with the final in Mumbai occurring shortly before the experiment. Especially a closely followed semifinal between India and its regional Muslim rival Pakistan exacerbated the communal tensions between Hindus and Muslims living in Mumbai. Many Hindus accused Muslims of cheering for Pakistan, which supposedly signaled their lack of loyalty to India.

Gujarat (10%) (MCGM 2010). Whereas Gujaratis (along with a small Parsi minority) are considered to be part of the city's traditional commercial bourgeoisie (Hansen 2001), more recent immigrants from North India are often frowned upon because of their high number (24% of all immigrants come from Uttar Pradesh) or their dramatically increasing immigration rate (especially those coming from Bihar). According to the census data, 19% of Mumbaikars are Muslims, though the number of Muslims living illegally in slums may be underreported.

Before describing the population of interest, let us briefly stop to explain the Indian caste system. In each linguistic region of India, there are up to 200 different castes called *jatis* (Ghurye 1969). Traditionally, castes were endogamous communities characterized by a hereditary occupation (e.g. washing clothes for Dhobis), ritualized behavior (e.g. lacto vegetarianism of Deshastha Brahmins), and cultural self-policing through a *panchayat* (caste council). Castes differed in social status and formed local hierarchies that converged during the colonial period into the religiously inspired *varna* system, with a social status scale running down from upper-caste Brahmins to the untouchables (Sheth 1999). Independence of India brought the formal end of this social stratification. At the same time, the caste system became politicized, as the new state gave special entitlements to the traditionally backward castes, clustered under the labels of Scheduled Castes (SC), Scheduled Tribes (ST), and Other Backward Castes (OBC). Indian political parties also promoted a system of patronage based on caste membership and ethnic voting (Chandra 2004). In this way, the Indian caste shares political salience with the Western concepts of class and ethnic group.

During the field research in the slums of Mumbai, I focused on the population of

mostly Hindu Marathi-speakers originally from Maharashtra. In accordance with the interchangeable use of these two terms in the environment, in which I conducted research, I refer to the population of interest as Marathi people or Maharashtrians. The Marathi community became the politically dominant group in the city after the creation of the separate Marathi-speaking state of Maharashtra with Bombay as its capital in the 1960s (Munshi and Rosenzweig 2006). As I explain in the paragraphs below, Marathi-speakers have a particularly salient social identity, defined in opposition to other ethnic groups and especially to Muslims. They are also the only ethnic group in Mumbai insisting on outsiders to use its language instead of the usual *Bambaiya Hindi* pidgin of Hindi, English, and Marathi that Salman Rushdie described skillfully as "Bombay's garbage argot… in which a sentence could begin in one language, swoop through a second and even a third and swing back round to the first" (Rushdie 1999). Clear ethnic boundaries made it easy to differentiate between ingroup and outgroup members during the experiments (Chapter 3) and survey (Chapter 4).

In Mumbai, the terms "Marathas", "Marathi people", and "Maharashtrians" are usually used interchangeably. It is not always clear whether they refer to the Marathi language group or the specific Maratha *jati*, descending from Marathi-speaking warriors and land-owning peasants of the Deccan Plateau. The ancient society of the Deccan Plateau was organized around clans that shared the same Marathi linguistic identity and a relatively homogenous culture (Hansen 2001). The broad linguistic identity was later stabilized "by references to *savarna jati*, the caste communities of pure blood, or the *hindu samaj*, the larger Hindu community as opposed to Muslims" (Hansen 2001, 24). The term "Maratha" originally denoted all Marathi-speakers and was only later defined

somewhat more narrowly in opposition to Brahmins (O'Hanlon 2002). The boundaries of the Maratha identity were traditionally more fluid than boundaries of more conventional castes. Historically, there has been a great deal of social mobility between the castes of Marathas, Kunbis, and Kolis, for instance. It was possible to become a Kunbi just by taking up an occupation in agriculture and, conversely, wealthy Kunbis could easily become Marathas (O'Hanlon 2002). Even now, many Kunbis and Agris have the same last names as and can pass for Marathas.

Residents of Mumbai often regard the Marathi category as encompassing other castes. When explicitly asked about their *jati*, some participants in my research declared themselves to be “Marathi (Koli)”, though Kolis are technically a separate *jati*, or even “Marathi (SC)”, though Marathas are a middle caste, while SCs are members of lower castes. Adding to confusion, one person from the Bhandari caste identified himself as a “Marathi (Bhandari)”, while another as just a “Bhandari”. Some members of the Maratha caste used a name of one of the 96 historical Maratha clans (e.g., Thakur) or a regional sub-group (e.g., Ghati) as their supposed *jati*. Among the Hindus who were sampled for this research and reported their caste, 65 percent identified themselves in one way or another as members of the Maratha caste.

Marathas are the largest supporter of both the mainstream Indian National Congress party and the nationalist political parties Shiv Sena and Maharashtra Navnirman Sena (Palshikar and Deshpande 1999; Palshikar 2004; Shaban 2010). The Shiv Sena movement was founded in Bombay in 1966 as a vehicle for the interests of the Marathi-speaking middle class against skilled South Indians (Hansen 2001; Palshikar 2004). Just like fascist parties in the interwar Europe, this anti-establishment social

movement employed marginalized young men from lower classes to terrorize its opponents. Over the years, Shiv Sena's targets changed from South Indians to Communists to the city elite to Indian Muslims (Hansen 2001).

Apart from the Maratha caste, Shiv Sena also finds a lot of support among Kunbis (Palshikar and Deshpande 1999) and OBCs (Palshikar 2004). Unlike elsewhere in India, OBCs in Maharashtra do not have any united identity. During the riots of 1987, Bombay's OBCs were part of Shiv Sena's rioters against low-caste Dalits (Hansen 2001). The fisherman caste of Kolis, the original inhabitants of what is now Mumbai, are also frequent members of Shiv Sena (Weinstein 2009). The Dalit vote is divided between different parties. Although they tend not to support Shiv Sena (Palshikar 2004; Vora and Palshikar 2005), Dalits sided with other Hindu rioters during the bloody ethnic riots in the 1990s (Vora and Palshikar 2005). All these castes share the same Marathi language, the same origin in Maharashtra, and, apart from an exception explained below, the same Hindu religion. Among the research participants who voted in the last *Lok Sabha* election, 40 percent chose Shiv Sena or its breakaway party Maharashtra Navnirman Sena (MNS).

In a vain attempt to escape the rigid caste system, a section of the low-caste Dalits (formerly known as untouchables) converted from Hinduism to Buddhism along with their leader B. R. Ambedkar in the mid-20th century. However, the religious boundaries between Hinduism and Buddhism in Mumbai are somewhat blurry and Buddhists are usually considered part of a larger Dalit community. Among the sampled slum-dwellers, most self-reported Buddhists identified themselves also as members of the Scheduled Castes. Some other respondents chose Hinduism as their religion, but then wrote

"Buddhist" when the questionnaire asked about their *jati*. During the bloody riots in the 1990s, Dalits sided with other Hindu rioters, though they normally do not vote for Shiv Sena (Vora and Palshikar 2005) and they still tend to support Ambedkar's party RPI (Shaban 2010). 86 percent of the participants in the research were Hindus, while the rest were mostly Buddhists. I kept Buddhists in the sample because, as Chapter 3 shows in more detail, they did not react to the experimental intervention differently from Hindus.

Mumbai's population density is the highest among all cities on Earth. There are only 21 square meters per person in the city of Mumbai – and this number includes not only apartments, but also all the roads, parks, office buildings, factories, and landfills. In comparison, New Yorkers enjoy 94 square meters per person despite living in a much more vertical city. Although the estimated six to seven million slum-dwellers compose more than half of the population of Mumbai, they are sardined into overcrowded areas that cover no more than 6-10 percent of the city's total land (Weinstein and Ren 2009).

According to realistic group conflict theory, hostility between groups arises when they compete for scarce resources and their interests are incompatible (Sherif et al. 1961). In Mumbai, the scarce resources of housing and employment have become a frequent point of contention between ethnically based political parties and patronage networks. Quillian (1995) argued that prejudice is driven by the dominant group's perception that the subordinate outgroup is threatening its economic conditions. Mumbai's dominant ethnic group indeed counters the perceived economic threat by a wide range of discriminatory activities, from political pressure to assassinations. Two nationalist political parties in Mumbai, Shiv Sena and MNS, have marketed themselves from the beginning as defenders of the "indigenous" Marathi-speaking people against South

Indians (Shiv Sena) and North Indians (MNS) in competition for jobs. Shiv Sena created an affiliated organization Sthaniya Lokadhikar Samiti (SLAS) to ensure that 80% of the most lucrative jobs are reserved to Maharashtrians. One of the managers complained about its influence (Noronha 2005, 122): “You cannot disallow the SLAS from recruiting their ‘own’ labour. They can go to the extent of killing you.” In accordance with the decision to choose the least-likely case, an experimental intervention in a laboratory may not be able to bridge the deeply entrenched ethnic cleavages based on conflicting material interests.

At the same time, the inevitable spatial proximity of different groups creates a quirky mix of competition and cooperation that we can find especially in slums, in which lower-caste Hindus from the Scheduled Castes mostly live next to and around Muslim ghettos (Shaban 2010). Equally discriminated against by upper-caste Hindus, both groups have developed many ties despite their fundamental religious difference. Katherine Boo illustrates an uneasy character of the Hindu-Muslim cooperation in Mumbai slums in her nonfiction book *Behind the Beautiful Forevers*. Asha, a Hindu woman and a local Shiv Sena operative, was trying to help her Muslim neighbors avoid jail. At the same time, she charged an exorbitant fee for her “brokerage” between a poor Muslim family and the predominantly Hindu police (Boo 2012).

I sampled only Marathi-speaking men. In Mumbai – and arguably also in general – men are more violent than women. It is no coincidence that the core electoral base of the nationalistic alliance of BJP and Shiv Sena in Maharashtra came from the urban male youth (Palshikar and Deshpande 1999). If we can reduce discrimination among men, the same effect will be even more plausible among women. The theoretical argument itself

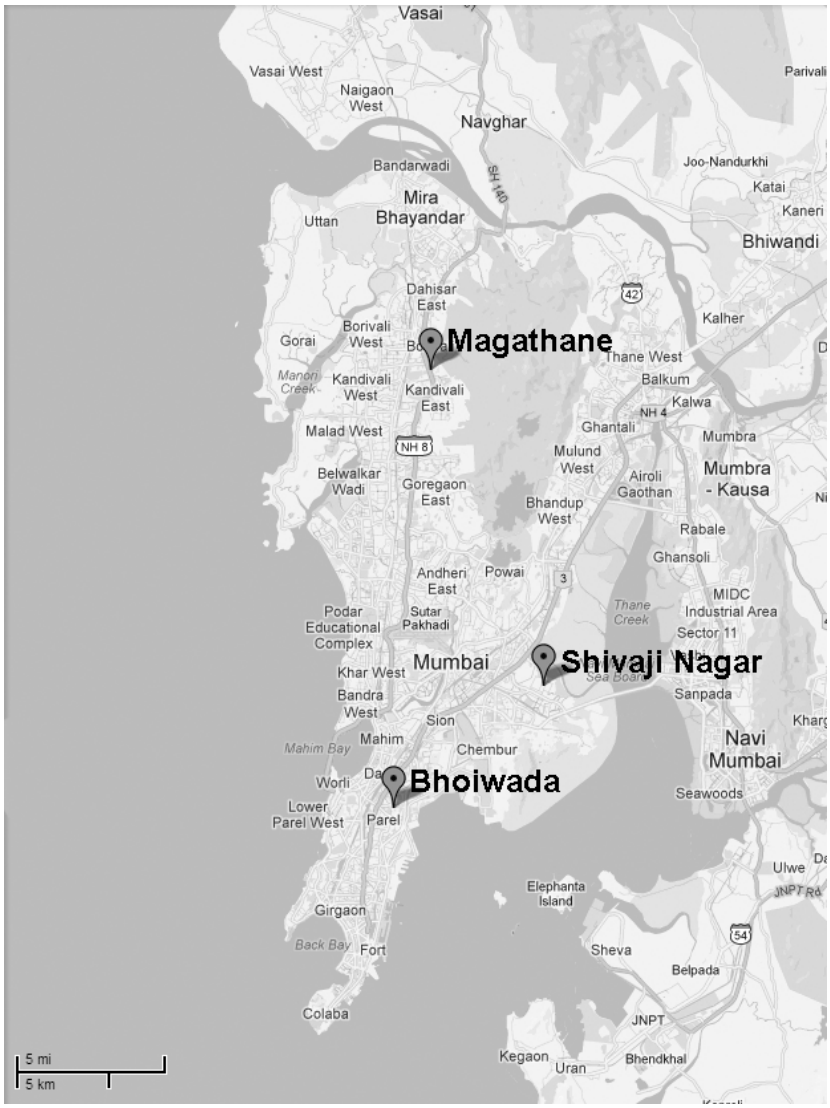
does not make different predictions for men and women. And if the predicted effect is a causal mechanism underlying the contact hypothesis, gender differences are unlikely because men and women displayed the same positive effect of intergroup contact on prejudice in earlier studies (Pettigrew and Tropp 2006).

2.3 Slums

There are approximately 2,000 slums in Mumbai (MCGM 2010). They vary greatly in size and have fuzzy borders. Although there are no reliable demographic data collected at the level of neighborhoods, local experts helped me identify three predominantly Marathi-speaking Hindu slums: Bhoiwada, Magathane, and Shivaji Nagar.

The three slums I chose are not only located in three different parts of the city (see Map 1), but their different levels of development encompass the whole spectrum of poor residential areas in Mumbai. *Bhoiwada* is the most "affluent" among the three neighborhoods. It is a typical example of an old industrial *chawl*. Chawls are single-room tenements, built in 1920-1950s by factories to house their workers. Most of them have two floors with shared toilets on each floor. Each small tenement now houses a whole (and often extended) family. Since rents were frozen by the Rent Control Act during World War II and never unfrozen again, landlords virtually abandoned any effort to maintain the buildings, creating slum-like living conditions (MCGM 2010). In comparison to Bhoiwada, Magathane and Shivaji Nagar are closer to the iconic image of an Indian slum, with independently built small huts. Many residents there rely on shared water sources and distant public toilets installed by local NGOs.

Map 1: Research Sites in Mumbai



As is often the case in Mumbai – and in other ethnically heterogeneous parts of the world –more or less homogenous buildings or streets lay in close proximity to zones inhabited by other ethnic groups. Shivaji Nagar with its alternation of exclusively "Hindu" and "Muslim" streets greatly illustrates this pattern. Although all the buildings in Shivaji Nagar are the same corrugated iron huts, the ubiquitous saffron *or* green flags easily point a lost visitor to the right ethnic bastion. The much smaller Magathane slum

borders a large slum inhabited by various ethnic groups from North and South India. Only Bhoiwada is ethnically homogenous, surrounded by other Maharashtrian areas. But even here, residents come from different castes, with a large number of SCs living together with Marathas.

As an official report concluded, “slums have a tendency of magnifying small issues and minor irritants into full–blown communal riots” (Srikrishna 1998). It is no coincidence that the very first serious ethnic riot in Bombay (in 1893) originated in a slum (Hansen 2001). Bhoiwada was one of the loci of serious rioting, stabbings, and attacks against madrassas in 1992-3 (Srikrishna 1998). In Shivaji Nagar, a rampaging mob killed constables “with choppers and swords,” and burnt down several temples during the riots (Srikrishna 1998). Local violent nonstate actors, whether it be gangs in Muslim slums or extremist political parties in Hindu slums, enjoy impunity in their respective areas (Shaban 2010).

At the same time, common civic institutions are securing communal peace in and between these neighborhoods. After the deadly riots in 1993, each police station has set up a *mohalla committee* consisting of local community leaders, who diffuse intergroup tensions at the time of crisis. Thakkar (2004) credited mohalla committees with prevention of ethnic riots in the wake of terrorist attacks in New York (9/11) and New Delhi, as well as after other incidents. During the terrorist attacks in Mumbai in 2003 and 2006, 26 members of a mohalla committee in the neighborhood of Nagpada reportedly spread out to calm members of their respective ethnic communities, put up soothing messages at street corners, and fought rumors that might have otherwise triggered a riot (Fernandes 2009). Although some mohalla committees have since then become mere

tools in the hands of the police or ambitious politicians, many other still serve their primary purpose of building bridges between rival ethnic communities (Thakkar 2004).

Security is not the only public good requiring bottom-up contributions channeled through the civil society. The main vehicle for public goods provision in the slums of Mumbai is *community-based organizations* (CBO). For example, slums located on hillsides get their water not directly from the authorities, but through their own cooperative societies that maintain pumps, collect money from members, and pay bills to the municipal corporation (MCGM 2010). Any break-up of cooperation results in a significantly lower quality of life for everyone: according to Gadpale (2011), women living in Mumbai's slums spend on average three hours every day walking to fetch drinking water and 69 percent of children skip school classes for the same purpose.

However, in order to succeed, CBOs have to coordinate hundreds of residents living in the same, ethnically mixed neighborhood. For example, an average citizen group involved in waste management covers almost 600 residents (Rathi 2006). The CBO has not only to collect garbage from households, but also to persuade households to separate “dry” and “wet” waste. Predictably, non-participation of residents is the biggest problem that CBOs face (Rathi 2006). To illustrate graveness of this issue with numbers, Bhide (2011) reported that only 3,000 out of 5,000 households in the neighborhood of Ketkipada paid their dues for garbage disposal, even though financial contributions were very modest (Rs. 10-20 per month per household, which is approximately 19-38 U.S. Cents). Public goods provision in Mumbai’s slums depends to a great degree on the local communities’ ability to cooperate. However, mistrust of other ethnic groups can easily hinder cooperation in ethnically diverse areas.

2.4 Sample

Public goods experiments are usually run in university laboratories, with subjects drawn from Ivy League undergraduate students taking courses in psychology or economics. According to Druckman et al. (2006), 64% of the laboratory experiments that have appeared in the *American Political Science Review* used student convenience samples. In *Political Behavior* and *Political Psychology*, this number raises to 70% (Kam et al. 2007), a figure similar to top-tier social psychology journals (Sears 1986; Sherman et al. 1999). If we consider only experimental studies of prejudice, one of the topics that I am focusing on in this study, 92% of the research articles published in top-tier social psychology journals used students as subjects (Henry 2008). Similarly, among 60 papers published in the major experimental economics journals in 2001-2002, only four did not use student samples (Danielson and Holm 2007).

Some of the leading experimentalists are wary of student convenience samples. In the words of Henrich (2001, 414):

> "There's three billion potential adult subjects out there who have never participated in any experiments, and are a lot more representative of humanity then the privileged inhabitants of elite universities. Researchers do not need to go to the Amazon, Papua New Guinea, the Ituri Forest, or Kalimentan—try the bus station, the beaches, the market, the used furniture auction, the bowling alley, the 'projects', or the county fair."

Because of systematic differences between American college students and the rest of the world, it is usually problematic to generalize results to any meaningful population.

As David Sears observed in his influential critique of student samples, "compared with older adults, college students are likely to have less-crystallized attitudes, less-formulated senses of self, stronger cognitive skills, stronger tendencies to comply with authority, and more unstable peer group relationships" (Sears 1986, 515). As a result, experimental studies using exclusively students may put too much emphasis on cognitive processes and too little on personality dispositions, material self-interest, emotionally based irrationalities, group norms, and stage-specific phenomena (Sears 1986). While comparing how students and nonstudents living in Oxford played typical experimental games, Belot et al. (2010) found that students followed more closely predictions of game theory, while nonstudents were more motivated by other-regarding preferences, such as altruism or trust. Henry (2008) found that students are less conservative, more egalitarian, and significantly friendlier to outgroups than the average member of the population. Contact with outgroup members also seems to reduce prejudice more easily among college students than among adults (Pettigrew and Tropp 2006). At the same time, Paluck and Green (2009) identified a dearth of prejudice-reduction studies among people who actually hold prejudiced beliefs.

In order to increase external validity of my findings, I chose a less common approach – random sampling of subjects representative of a politically relevant population of slum-dwellers, many of them voters of extreme nationalist parties infamous for organizing ethnic riots.[5] The experiments were conducted in rented Internet cafes

[5] Another advantage is that, unlike people sampled from usual subject pools (such as at universities or over the Internet), slum-dwellers had no prior experience with behavioral experiments. As there was only one experimental manipulation involved, the design eliminated "cross-effects" occurring "when subjects' choices in an experiment are influenced by the manipulations they have received in previous experiments" (Morton and Williams 2010, 315).

close to the three slums. Combining advantages of field experiments (a representative sample) and lab experiments (full control over the environment), this approach is called a "lab in the field" experiment (Morton and Williams 2010). Hopefully, my experimental study of real people living in violence-prone slums of Mumbai will help remedy the current state of our field, in which "there is little sustained experimental evaluation of conflict negotiation and reduction for the many millions of ordinary citizens living in conflict or postconflict settings" (Paluck and Green 2009, 359).

Inferences from a random sample can be generalizable to the population as a whole. Ideally, a random sample would be drawn from a list of all eligible individuals living in the area. Unfortunately, no such list exists in this case. Due to the questionable legality of residence in slums, not even voting registers are very reliable. Furthermore, slums themselves resemble an ever-shifting maze of unnamed streets and alleys that would make finding each of hundreds of registered voters sampled from a voting register an insurmountable logistical challenge.

Instead, the research team used a "random route" technique to sample households in regular intervals while walking in a randomly selected direction in the sampled area. Once a household has been selected, a full list of all eligible individuals (adult Marathi-speaking males) was written down and one of them randomly chosen. If the chosen individual refused to participate or was not present in the household, one more attempt to reach him was made in a prearranged time. In case the second attempt failed, substitution by another eligible individual from the same household was permitted.

40 percent of the initially sampled individuals in the slums of Shivaji Nagar and

Magathane, and 20 percent in Bhoiwada refused to participate in the research.[6] As random assignment to treatment groups happened only after subjects agreed to participate, refusals should not bias the experimental results reported in Chapter 3. However, refusals may limit external validity of the study. Although no systematic survey of the refusals could be made, the socioeconomic conditions of those who did and those who did not participate seemed to be virtually the same to the interviewers. The only reason consistently mentioned by those who refused to participate was lack of time.

Although randomization of treatment assignment prevents bias in the reported experiments, potential bias could be introduced to the observational part of the research (Chapter 4) if one of the key variables was also a reason why some people refused to participate (Groves 2009). For example, a sensitive question about discrimination could motivate people with discriminatory attitudes not to participate at all. Since all the key questions in the survey elicited a response rate close to 100%, it is unlikely that the refusals were motivated by fear of answering sensitive questions. In fact, slum-dwellers seemed to be happy to share their opinions – even those usually considered extreme outside India – with a white researcher.

The resulting sample includes 402 adult male Marathi-speaking slum-dwellers – 134 in each neighborhood.[7] The average age is 31 years. 86 percent of the sample are Hindus, while 13 percent are self-reported Buddhists. 98 percent speak Marathi at home and only two subjects were not born in Maharashtra. With the participants having

[6] For comparison, the no-show rate in experiments run by economists at Western universities is around 30% (Friedman et al. 2004).

[7] The number of subjects was predetermined by power analysis and available resources. Cornfield (1966) warns against ad hoc stopping rules that can be used essentially to halt data collection as soon as statistically significant results are obtained. In order to avoid this temptation, I did not conduct any analysis until all the data were collected.

completed almost 11 years of school on average, the literacy rate in the sample reached 96%. The average household income varies a lot across neighborhoods, from Rs. 34,000 (about $640) a month in Bhoiwada to Rs. 16,000 ($300) in Magathane to Rs. 13,400 ($250) in Shivaji Nagar. In accordance with the logic of the least-likely crucial case, journalists and party operatives with whom I talked identified all three neighborhoods as traditional regional bastions of the extremist parties Shiv Sena and MNS. However, a survey administered for this study offers a more refined picture: among those, who voted in the last *Lok Sabha* elections (2009), 40 percent chose Shiv Sena or MNS, while the rest supported other political parties.

Chapter 3. An Experiment of Hindu-Muslims Cooperation in Slums

The main goal of the experiments described in this chapter is to find out whether indirect reciprocity has an effect on trust and discrimination. Although the tradition of experiments in political science goes back to the 1920s, experimental methods have come to prominence only recently. The total of experimental publications in the *American Political Science Review*, the *American Journal of Political Science*, and the *Journal of Politics* in the first five years of the 21st century equals the number of experimental articles published in the whole 1990s and is twice as high as the number of articles in the 1980s (Morton and Williams 2010). Political scientists use experiments more and more often as the field is slowly moving from studying correlations to the deeper and more interesting questions of causal inference. An experimenter can control confounding factors that are normally impossible or at least very difficult to disentangle from the explanation of interest in other research designs.

Another advantage of the experimental research design is that it is less prone to the endogeneity problem. Most if not all relevant explanatory variables in political science are arguably endogenous to the outcome that they seek to explain. Institutions surely do influence behavior, but were they not built by actions of the same actors at the first place? An experimenter can exogenously determine institutions and randomly assign subjects to the groups with different institutional settings.

Despite all these advantages, most studies of prejudice, discrimination, and related topics still heavily rely on non-experimental methods or have questionable internal validity. Out of 985 studies on how to reduce prejudice collected by Paluck and Green

(2009), 60% are non-experimental. As Paluck and Green (2009, 345) concluded, "fewer than twelve [out of 985 studies] can be considered strongly suggestive of causal impact (or lack thereof)." Focusing heavily on unbiased causal inference and using qualitative evidence to assess internal validity, this dissertation seeks to solve some of the biggest problems with studies of prejudice.

3.1 Procedure

3.1.1 Randomization

The research team sampled randomly 402 adult male Marathi-speaking slum-dwellers – 134 in each neighborhood. Among them, 210 randomly selected individuals were interviewed, but they did not receive any experimental treatment. Following the usual practice in the field, I refer to them as to the "control group" because their attitudes reflect the baseline values in the population. At the same time, the control group is not a true placebo group as in medical research because it is different from the treatment groups in several regards simultaneously.[8] Therefore, the quantitative analysis presented in this chapter focuses mostly on comparisons across four treatment groups, while the next chapter looks at the respondents assigned to the control group.

Subjects in each area were first randomly assigned to either the control group (and were interviewed) or to receive the treatment (and arranged an appointment for an

[8] In contrast to the treatment groups, subjects in this group did not play behavioral games. They knew that they would not receive any monetary reward for their participation in the survey. They were interviewed by an interviewer instead of filling a questionnaire in on a computer. Interviewers were other Hindu Marathi males in order to reduce social desirability bias.

experimental session, usually on the same or next day). At the beginning of each experimental session, a random number generator assigned subject to one of the four treatment groups. As I describe in more detail later in this chapter, four treatment groups correspond to the four cells of Table 1. The experiment used block – not cluster – randomization, with treatment assignment randomized within each neighborhood. Within neighborhoods, I used the simple randomization. As a result, each individual had the same probability of receiving one of the treatments. Table 2 demonstrates that treatment assignment did not correlate with location and the treatment groups were well balanced.

Table 2: Sampled Individuals by Treatment Group and Neighborhood

Treatment Group	Bhoiwada	Magathane	Shivaji N.	Total
“Generalized Reciprocity” Group	14	17	17	48
“Cooperative Hindus” Group	15	16	16	47
“Cooperative Muslims” Group	16	17	16	49
“No Reciprocity” Group	19	14	15	48
Control Group	70	70	70	210
Total	134	134	134	402

Despite some observable differences between treatment groups (see Table 3), imbalances in demographic characteristics were not statistically significant at the usual .05 level except for age, with subjects in the control group on average slightly older than subjects assigned to three out of four treatment groups. However, even in this case, treatment assignment explains less than 3 percent of variation in age.

Table 3: Demographic Characteristics of the Treatment Groups

	Generalized Reciprocity	Cooperative Hindus	Cooperative Muslims	No Reciprocity	Control Group
Hindus (%)	81 (5.6)	81 (5.7)	82 (5.5)	81 (5.6)	90 (2.1)
Average Age	28 (1.2)	29 (1.3)	28 (1.4)	29 (1.4)	32 (0.7)
Years of Schooling	10 (0.7)	10 (0.8)	11 (0.6)	10 (0.7)	11 (0.2)
Household Income (Rs.)	15,798 (2,050)	32,489 (7,967)	29,958 (6,937)	20,446 (3,875)	17,856 (1,436)
SHS and MNS Voters (%)	21 (5.9)	28 (6.5)	29 (6.5)	44 (7.2)	38 (3.3)

Design-based robust standard errors in parentheses.

3.1.2 Laboratory Experiments

During the experiment, each subject assigned to one of four treatment groups was seated in front of a computer. He learned that he would play simple computer games over the Internet with various people in two other areas of Mumbai: Bhendi Bazar (a typical Muslim enclave) and Dadar (a well-known Hindu Marathi neighborhood). Although ethnic affiliation was never explicitly mentioned during the experiment, the computer screen showed a photograph, the first name, and the neighborhood of the other player – three unobtrusive cues of the partner's membership in the Hindu ingroup or the Muslim outgroup. The research team explained to subjects that their partners could see the name of the subject's neighborhood (a cue for ethnicity), but not his face or name. Therefore, the partner supposedly knew that the subject was a Hindu, but, at the same time, there

was no fear of retaliation against the subject as an individual person.

3.1.2.1 First Dictator Game

Cooperation, including joint provision of public goods, cannot be explained by material benefits and costs alone. In addition to cost-benefit considerations, it is motivated by other-regarding preferences, such as reciprocity and altruism (Meier 2006). Much empirical research shows that true altruism, distinguishable from material benefits and social pressure, exists and is part of the human nature (Dovidio 1984; Piliavin and Charng 1990).[9]

This additional intrinsic psychological motivation consists of two different elements: General altruism is independent from the recipient's identity, while the element that I call *ethnocentric altruism* gives more value to cooperation with ingroup members than with outgroup members. Evolutionary biologists believe that ethnocentric altruism has a biological basis and that generosity reflects kinship ties between the donor and the recipient (Hamilton 1996). It is a well-observed fact that even non-biologists are more willing to help their kinsmen rather than strangers (Cialdini et al. 1997). However, the term ethnocentric altruism is a bit misleading because we tend to help not only people who are genetically related to us, but also those who are similar to us in other regards (Dovidio 1984), or whom we like for whatever reason (Mallozzi et al. 1990).[10] In order to

[9] Of course, altruism can be egoistically motivated (Meier 2006), for example by social approval (Deutsch and Lamberti 1986), or – as trivial as it sounds – enhanced mood (Gueguen and Gail 2003).

[10] For example, an amusing experiment reported in Mallozzi et al. (1990) demonstrated that a well-dressed woman elicited more helping behavior from drivers passing around than male confederates.

identify the causal effect of reciprocity, the research design differentiates between reciprocity, general altruism, and ethnocentric altruism. For this purpose, it uses the dictator game.

After receiving the instructions and a brief tutorial about computer use, subjects played one round of a dictator game. The dictator game is a one-stage game in which a subject decides how to allocate a sum of money between herself and other players (hence the “dictator” name). A purely self-interested person should keep all the money. However, empirical research found that people usually transfer considerable amounts of money to other players (Camerer 2003). Participants in my study played a version in which the allocator is anonymous to other players, but, at the same time, the allocator knows the receivers’ identities. Manipulating the information available to the allocator, we can not only measure the allocator's baseline altruism (Camerer 2003), but also altruism towards different groups (Bohnet and Frey 1999; Fershtman and Gneezy 2001; Habyarimana et al. 2009), which is the goal of this part of the research.

During the dictator game, each subject was given 10 Rupees and told that he could divide them between himself and two partners in any way he wished. The two partners were represented on the computer screen by a photograph, first name, and neighborhood. Using these clues, one partner appeared to be a Marathi Hindu (an ingroup member) and the other a Muslim (an outgroup member). The amounts of money allocated by the player to each of them create direct behavioral measures of the subject’s baseline altruism in relation to ingroup and outgroup members. The money retained by the subject is an inverse measure of general altruism.

3.1.2.2 Public Goods Game

I employed the *public goods game* to manipulate expectations of cooperative behavior during the experiment. This behavioral game is used frequently by economists – see Camerer (2003) for a review – and increasingly also by political scientists to investigate under what conditions people cooperate. In the public goods game, two or more subjects secretly and simultaneously choose how many tokens to contribute to a common pot. The experimenter multiplies the pot and distributes the resulting amount equally among all the subjects regardless of their contributions. This procedure gave the public goods game its name. Subjects also retain privately all the tokens they did not contribute. Unlike other experiments that use the public goods game merely to measure the cooperation rate between subjects, I used the game as a randomized treatment inducing group-specific expectations of cooperative behavior.

3.1.2.2.1 Logic of the Payoff Structure

The payoff structure of the game played by the participants (Table 4) corresponds to the s*tag hunt game* that got its name from a famous story in Jean-Jacques Rousseau's *Discourse on the Origin and Basis of Inequality Among Men*. The story, never actually spelled out by its author, can be envisaged as follows: Two or more hunters are hunting a stag. While they are waiting for the beast to appear, one of the hunters sees a hare passing by. The hunter faces a dilemma: If he shoots the hare, he will get its meat for himself. Unfortunately, the shot will also scare the stag off. The stag could provide him and his

companions with much more meat than a single hare. However, if the hunter decides to wait for the stag, he risks that someone else will see the hare and fire at it. In its general form, the stag hunt game can be described by the following ordering of outcomes: $CC > DC \geq DD > CD$.

Table 4: Payoff Structure of the Stag Hunt Game

		Player 2	
		Cooperate	Defect
Player 1	Cooperate	20 20	10 0
	Defect	0 10	10 10

The game has two pure-strategy Nash equilibria: Either both players cooperate or they both defect. How the game will end depends on the level of trust in the group. A rational player will hunt the stag if she expects the other player to do the same; but she will go for the hare if she does not trust her companion. Both players are better off if they hunt the stag (CC); this is the Pareto-optimal equilibrium. But to hunt the hare (DD) is a risk-dominant strategy. There is also a mixed strategy equilibrium depending on the exact payoff matrix. In the game used in India, the mixed equilibrium was to cooperate with the probability of 0.5.

No player can provide the public good alone. This feature corresponds to the sociological definition of social exchange, whose purpose is “to generate benefit for each individual by exchanging behaviors or goods that actors cannot achieve alone” (Lawler

2001, 322). Situations, in which no cooperation is actually needed because one of the players can produce the public good alone are outside the scope of my work.

Most laboratory public goods games to this day have used the prisoner's dilemma payoff matrix. The key difference is that gains from mutual cooperation (CC) are higher than gains from exploitation (DC) in the stag hunt, while the opposite is true for the prisoner's dilemma (Jervis 1978). Although a rational egoist should always defect in a one-shot prisoner's dilemma game, real people tend to cooperate if they believe that others are likely to cooperate (Ostrom 2000). For example, even in a one-shot prisoner's dilemma game, 40% of subjects actually preferred (CC) to (DC) and 27% were indifferent between these two alternatives (Ahn et al. 2001). Even if material payoffs are structured like the prisoner's dilemma, other factors, such as other-regarding preferences, are likely to transform the game into a stag hunt.

The stag hunt game was hailed as the "prototype of the social contract" (Skyrms 2004, 1). As early as in the 18th century, David Hume used the stag hunt game to model public goods provision. In his example, two neighbors wish to drain a meadow that they own in common. If they both work hard, they will succeed. But if either fails in his part, the meadow will not be drained (Hume 1739 [2003]).

In his example of two neighbors draining a common meadow, Hume (1739 [2003]) recognized that achieving cooperation is more difficult in many-person stag hunt games than in a two-person game:

> "Two neighbors may agree to drain a meadow, which they possess in common; because it is easy for them to know each others mind; and each must perceive, that the immediate consequence of his failing in his part, is, the abandoning the

> whole project. But it is very difficult, and indeed impossible, that a thousand persons should agree in any such action; it being difficult for them to concert so complicated a design, and still more difficult for them to execute it; while each seeks a pretext to free himself of the trouble and expence, and would lay the whole burden on others."

Hume's intuition was formalized in the 20th century. According to Oye (1985), cooperation decreases as the number of players increases due to higher transaction and information costs, and the difficulty of anticipating other players' future behavior. As players are less likely to achieve the potential gains from cooperation (CC), a stag hunt game can turn into a less beneficial prisoner's dilemma (Jervis 1978). Mutual defection is the unique stochastically stable equilibrium in the stag hunt game (Skyrms 2004) – a result reminiscent of the prisoner's dilemma.

Mumbaikars play n-person stag hunt games every day. For example, if local residents want to have garbage removed from their streets, they have to put together a modest fee. If fundraising succeeds, everyone is better off. However, this simple act of cooperation requires monetary contributions from hundreds of households living in the same, often ethnically heterogeneous neighborhood. If residents do not trust their neighbors because of caste or religious differences, and consequently expect fundraising to fail, they have little incentive to contribute. The neighborhood descends to a state of disorder, in which local women spend their days trying to move garbage from near their houses to the vicinity of their neighbors' houses – or deterring such attempts by their neighbors.

Hume (1739 [2003]) argued that an obvious solution to the tragedy of commons

in n-person stag hunt games is creation of the state or other political institutions able to coordinate cooperation between selfish and distrustful individuals:

> "Political society easily remedies both these inconveniences. Magistrates find an immediate interest in the interest of any considerable part of their subjects. They need consult no body but themselves to form any scheme for the promoting of that interest. And as the failure of any one piece in the execution is connected, though not immediately, with the failure of the whole, they prevent that failure, because they find no interest in it, either immediate or remote. Thus bridges are built; harbours opened; ramparts raised; canals formed; fleets equiped; and armies disciplined every where, by the care of government, which, though composed of men subject to all human infirmities, becomes, by one of the finest and most subtle inventions imaginable, a composition, which is, in some measure, exempted from all these infirmities."

From the evolutionary perspective, cooperation in the n-person stag hunt game can be self-sustained in the long run if the initial proportion of cooperators is high enough (Skyrms 2004). If a sufficient number of people hunt a stag from the very beginning, they will eventually take over the whole population, as the less efficient hare hunters will die of. More complicated setups may lead to cooperative evolutionary stable equilibria if cooperators interact more frequently with each other (for example within a social network), if they are allowed to send credible signals about their type, or if they can learn from the past through positive reinforcement (Skyrms 2004). As members of the person's group can learn about her past more easily than outsiders (Fearon and Laitin 1996; Habyarimana et al. 2009), cooperative solutions to the n-person stag hunt game are

probably more likely to emerge in bounded groups. My theory suggests that intergroup contact can enable coordination between cooperators across ethnic boundaries as well.

Another way to induce more cooperation is to play an iterated version of the game. When people face situations analogical to the stag hunt or prisoner's dilemma games repeatedly and value future gains, the “shadow of the future” leads to more cooperation (Axelrod 1984; Oye 1985). In fact, even the prisoner's dilemma can become much more similar to the stag hunt as the shadow of the future reduces the relative payoff of exploitation (Jervis 1978). Since this mechanism is different from the one tested in the dissertation, participants in the Mumbai experiment knew that they would face each partner only once. Unlike in Axelrod’s tit-for-tat strategy, reciprocity was indirect, not direct.

The experiment models simple dyadic interactions in order to establish a clear causal relationship between reciprocity and discrimination. This study will hopefully become a stepping stone for subsequent research that will explore updating of stereotypes in larger groups. Previous studies of n-person stag hunt games suggest that success of real-world interventions based on the proposed theory will probably depend on the social and political context. On the one hand, it may be harder to encourage cooperation among a large number of people than it is among two subjects inside a lab. On the other hand, pre-existing institutions, prior intergroup contact, and mutual interdependence in the real world may facilitate intergroup cooperation.

Interactions between players and their partners in the experimental public goods game were anonymous. This feature makes the research design more appropriate if one seeks to understand intergroup relations in an urban setting, such as slums (Habyarimana

et al. 2009). On the other hand, processes in small-scale rural communities based on repeated face-to-face interactions may work differently. The dissertation's inferences are also relevant to intergroup relations at the national or regional level, where sheer size of communities precludes repeated face-to-face interactions with most members. Given the fact that people in most modern societies identify themselves with and classify others into large cultural categories, the "imagined communities" of nations (Gellner 1983; Anderson 1991), the model may help understand many macroscopic phenomena in the realm of politics. In the end, examples of people cooperating with strangers encompass a large range of behaviors from paying taxes to voting to fighting wars.

3.1.2.2.2 Procedure of the Public Goods Game

Players participated in ten rounds of a public goods game, first with five distinct ingroup members, and then with five distinct outgroup members. Since players knew they would face the same partner only once, any reciprocity would be indirect, not direct. The setup follows the typical procedures in experimental economics according to which subjects perform the same task repeatedly, each time with fresh endowments, until learning makes their behavior converge at an equilibrium (Camerer et al. 2004).[11]

Participants were promised financial rewards contingent on their performance in the experimental games. Salient monetary rewards are a standard practice in economic

[11] Although more repetitions may produce more pronounced results, ten rounds generally seem to be sufficient to identify patterns in behavioral games. In their experiments, Fehr and Gächter (2000) used twenty rounds of the public goods game, while Cinyabuguma et al. (2005) fifteen. Andreoni (1995), Brandts and Schram (2001), Houser and Kurzban (2002), Nikiforakis and Normann (2008) used all ten periods. Andreoni and Petrie (2004) used only eight and Baldassarri and Grossman (2011) only six rounds. Ostrom (2000, 139) concluded that behavior in public goods games "closely approximates the predicted equilibrium by the end of the first five rounds."

experiments at least since Smith (1962) and experimental economists prefer them over survey questions that do not entail any cost for the respondent. In the words of a popular experimental economics textbook, “what people say they would do in hypothetical situations does not necessarily correspond to what they actually do” and, therefore, “an experimentalist who uses unmotivated subjects can anticipate that many economists will challenge the results” (Friedman and Shyam 1994, 15). A half of the experimental studies published in the *American Political Science Review* also used financial rewards (Druckman et al. 2006). The average compensation for a half an hour-long experiment in the slums represented approximately 20% of the average daily household income.

In each round, the player received 10 Rupees and could decide whether to keep them or to “invest them in a common project” with the partner on the screen. The partner made the same decision simultaneously, without any communication with the player. The payoff structure corresponded to the stag hunt game (Table 4): If both the player and his partner invested in the common project, each of them received 20 Rupees (CC). If the player invested, but the partner kept his money, the project failed and the player lost his money (CD). Alternatively, the player could simply keep the 10 Rupees. If he did so and the partner kept his money too, each got the payoff of 10 Rupees (DD). If the partner invested, but the player kept his money, the player’s payoff was still 10 Rupees, while the partner’s payoff was 0 (DC). The best strategy clearly depended on whether the player expected his partner to cooperate or not.

Subjects played each round with a different partner and did not know the partner’s previous record of cooperation. After each round, a screen showed the results of the round: how much the player invested, how much the partner invested, whether the

common project succeeded, the amount of money kept, and the amount of money gained from the investment. As none of the subjects have ever seen a behavioral game before, the research team explained the payoff structure carefully before commencing the experiment. We went through a number of examples on a blackboard and answered all the questions until we were certain that everyone in the room understood the game.

Four treatment groups correspond to the cells of Table 1. Subjects in the *Generalized Reciprocity* group always faced cooperators: first five Hindus, then five Muslims. Subjects in the *Cooperative Hindus* group played five rounds with cooperating Hindus and then five rounds with defecting Muslims. The *Cooperative Muslims* group was paired with uncooperative Hindus and cooperative Muslims. In the *No Reciprocity* group, all ten partners defected. The manipulated frequency of defection in the game created low or high expectations of cooperative behavior from Hindus and Muslims. These four ideal types cover the whole spectrum of possible intergroup interactions.

One can imagine similar experiments, in which ingroup and outgroup partners would appear in a random sequence and their probability of cooperation would be different from 0 *or* 100%. Although such a design would be more believable to seasoned subjects in Western laboratories, it would also effectively multiply the number of treatment groups. Strictly speaking, a subject facing four Muslim cooperators and one Muslim defector does not receive the same treatment as a subject facing five Muslim cooperators. A more realistic randomization would decrease statistical power because we would have to compare many more groups with fewer subjects in each.

31 percent of laboratory experiments in the *American Political Science Review* involved deception (Druckman et al. 2006). More than a half of published studies in the

field of social psychology also used deception (Druckman et al. 2006) and this tool is especially prevalent among studies of prejudice (McDermott 2002). My experimental design is no exception. The element of deception consisted in the participants believing that they were playing games with real people, while their partners were in fact generated by a computer program.[12] Unfortunately, we would not necessarily be able to observe people's behavior across all four experimental conditions without deception. The research aims to measure the causal effect of the treatment variable, not just to measure its preexisting levels and the resulting equilibrium behavior in the sample. Furthermore, as experiencing cooperation is the central explanatory variable here, I needed to randomize it across subjects to be able to draw unbiased causal inference. If I merely observed evolving unrestricted behavior among real pairs of people, I might have not been able to identify the average treatment effect (ATE) at all. Green and Tusicisny (2012) discuss limits of causal inference for an identical example:

> "Even a basic experimental design with only one partner, two possible outcomes (cooperate, defect), and 10 rounds of a public goods game creates 2^{10} possible trajectories. For any given subject, the true counterfactual would be a subject facing precisely the same sequence of ten partners' behaviors and differing only in treatment assignment. With 2^{10} theoretical possibilities and N typically in dozens or hundreds, it would be virtually impossible to find a meaningful pair of subjects to compare."

Although it sounds paradoxical, another reason for deception was ethical. After

[12] First names were coded independently by two local people as exclusively Hindu, exclusively Muslim, or other. Only the names consistently recognized as either Hindu or Muslim were used for the computer-generated "partners." Faces on the photographs belonged to either Marathi-speaking Hindus or Muslims living in a different town near Mumbai.

the debriefing, subjects knew that non-cooperative behavior was generated by a computer and was not a real attribute of the Muslim minority. Had they encountered real uncooperative Muslims during the experiment, the research itself could contribute to negative stereotyping of this group. I decided not to take this risk due to its potential ethical repercussions in a violent city like Mumbai.

Deception was explained to subjects during debriefing. Although subjects seemed to be genuinely surprised by this information, none of them reacted in a negative way. Fun and novelty greatly outweighed potential discomfort associated with deception. Since it would be unfair to pay a lower amount of money to those who had faced defectors, all subjects were given the maximum earning instead.

Among 192 participants who interacted with the computer, none expressed disbelief or doubts about existence of other players during the experiment or when asked about it during the debriefing. This is otherwise a frequent problem among “professional subjects” in developed countries (Morton and Williams 2010), who might have fund the homogenous behavior of their partners (all members of the same group either cooperating or defecting) more suspicious. Replication of this experiment in a seasoned subject pool will probably require a greater variation in the behavior, to which subjects are exposed.

There is no empirical evidence that experiments using deception are more harmful to the subjects than other experiments (McDermott 2002). However, economists are often concerned that participants in the experiments that involve deception will not trust other experimenters in the future (Morton and Williams 2010). As the likelihood that any of the sampled slum-dwellers will ever participate in a behavioral experiment again is microscopic, the sampling strategy prevents this so-called “pollution” of the sample pool

by deception (Henrich 2001).

The research design holds the potentially confounding effect of social sanctioning constant at zero. Subjects interacted only through computers. Their partners could not see their names or faces. Therefore, players had no reason to fear retaliation after the experiment ended. Privacy screens around computers assured anonymity inside the lab. In addition, subjects were not allowed to talk to each other or to look at each other's screen. As they could not copy someone else's strategy, the research design also minimized possible spillover effects that could otherwise lead to biased estimates of the ATE by violating the non-interference assumption, better known under the acronym SUTVA (Rubin 1990).

Both non-compliance and attrition rates were equal to zero. Subjects were motivated by nontrivial financial compensation and perhaps even more by an interesting and novel activity. During the debriefing, many participants expressed their gratitude not as much for the earned money, as for the first opportunity to use a personal computer in their lives. That is why they took the experiment seriously and complied with the instructions.

3.1.2.3 Second Dictator Game

In order to see whether the treatment changed other-regarding preferences, I repeated one round of the dictator game after the end of the public goods game. Subjects were asked again to divide 10 Rupees between themselves, an anonymous Hindu, and an anonymous Muslim.

3.1.3 Post-Treatment Survey

The subject's contributions to the common project serve as behavioral measures of cooperation. Most public goods game experiments stop here and content themselves with measuring the treatment effect on cooperation in an artificial laboratory setting. However, I also asked subjects about their real-life attitudes and behavioral intentions in a survey administered several minutes after the experiment.

In accordance to the usual practice in experimental economics (Berg et al. 1995; Cox 2004), I used trust as a proxy for stereotypical beliefs about cooperative behavior. Trust is also a major component of social capital (Putnam et al. 1993), defined as "institutionalized expectations that other social actors will reciprocate co-operative overtures" (Boix and Posner 1998, 686). According to Putnam et al. (1993) and others, interpersonal trust facilitates collective action, which is essential for public goods provision. In the words of Cox (2004, 263):

> "[Trust] is inherently a matter of the beliefs that one agent has about the behavior of another. An action that is trusting of another is one that creates the possibility of mutual benefit, if the other person is cooperative, and the risk of loss to oneself if the other person defects."

Cooperation in the public goods game indicates how much subjects trusted their individual Maharashtrian and Muslim partners to cooperate. The survey took one step further and asked about trust in Maharashtrians and Muslims in general. The survey measure of trust was a slightly modified Eurobarometer question: "I would like to ask you a question about how much trust you have in people from various groups. For each,

please tell me whether you have a lot of trust, some trust, not very much trust or no trust at all?" The subjects interviewed during the pretest understood the question unanimously as asking whether they would found a business partnership with or lend money to a Muslim.

Allport (1954) suggested that there are five ways how prejudice can be expressed in behavior: by antilocution (talking in terms of prejudice or making jokes), avoidance (avoiding contact with members of an outgroup), discrimination (actively doing something to deny members of an outgroup something they desire), physical attack (beatings, lynchings, etc.), and extermination (an attempt to eliminate an entire group). Allport (1954) also argued that softer forms set the stage for more serious expressions of prejudice. Discriminatory behavior often escalates, like in Nazi Germany, from avoidance to discrimination to physical attacks to extermination. As I could not observe the harshest forms of discriminatory behavior among the research participants, the survey asked about a softer form: avoidance. Tendencies to avoid contact with a member of a particular group are analytically interchangeable with social distance (Oskamp 2004). Accordingly, I used two questions from Bogardus' social distance scale (Bogardus 1925) as proxies for discriminatory attitudes. The first question asks: "If it was up to you, would you accept a Muslim as a neighbor?" The second question asks: "If it was up to you, would you accept a Muslim to close kinship by marriage?" The second question represents the extreme pole of Bogardus' scale because even the most tolerant respondents in liberal democracies often discriminate against outgroups as long as family matters are concerned. Bogardus' social distance scale is used often by psychologists. In the field of political science, Sniderman et al. (2004) measured social distance by asking how attractive an outgroup

member would be as a neighbor or as a life partner. A similar question about acceptance of neighbors is also part of the World Values Survey.

All the indicators of hostile behavioral intentions are explicit (i.e. self-reported in a questionnaire) and not implicit. An example of an implicit measure would be the popular implicit association test (IAT), described in Greenwald et al. (1998). I decided to use an explicit measure because it predicts discrimination in social life better than implicit measures of prejudices do (Maass et al. 2000). Implicit measures do not affect judgmental tasks requiring some cognitive effort and they predict primarily non-verbal behavior (Dovidio et al. 1997). In a large number of studies, implicit measures were at best a weak or noisy predictor of discriminatory behavior (Blanton and Jaccard 2008). Although explicit measures of racism do not always produce sufficient variation in North America and Europe, as we will see in the section discussing the experimental results, Hindu slum-dwellers were more than willing to admit their bias against Muslims.

Since Muslim players were allegedly from Bhendi Bazar, a control variable called "Bhendi Bazar" measured whether the subject had acquaintances in that neighborhood. I controlled for covariates, which appeared to be slightly imbalanced across treatment groups (see Table 3) in order to eliminate the observed differences between the groups: religious affiliation, age, number of years of schooling, household income, and voting for Shiv Sena or MNS in the last *Lok Sabha* elections.[13] I also added fixed effects for the neighborhoods. These control variables were measured by the post-treatment survey. Post-treatment measurement of covariates should be generally avoided because the variables may be endogenous to the experimental intervention itself (Robins and

[13] To facilitate interpretation of the regression coefficients, age, income, and the number of years of schooling are centered at their mean values. Income is measured in thousands of Rupees.

Greenland 1992). Nevertheless, it is difficult to imagine how the treatment could change subjects' fixed attributes in this study.

There is a disagreement in the literature about the virtue of control variables in randomized experiments. Maxwell and Delaney (2004) argued that inclusion of control variables will reduce bias if randomization is not perfect and increase precision even in a perfectly randomized study. But they also suggested that we should include only covariates balanced across treatment groups and covariates highly correlated with the dependent variable. Freedman (2008) cautioned that a multivariate regression may return biased estimates in small samples. So far, the last word in this debate is Green and Aronow (2011), who show that this bias diminishes quickly with the sample size and usually should not be a problem for samples over 20 subjects. Because of a relatively high N in this study, I decided to estimate the treatment effect using a multivariate regression with control variables. Covariates in regression models have an advantage that they lead to more precise estimates of the ATE. At the same time, the article also reports the regression coefficients without a covariate adjustment, as well as difference-in-means estimates of the ATE, in order to show that covariates do not substantively change the results (Gerber and Green 2012). Of course, other, unobserved confounding factors may also affect discrimination. Randomization ensures that they are orthogonal to the treatment and therefore do not bias the results. Design-based standard errors take into account randomization of treatment within three blocks (neighborhoods) and are robust to heteroscedasticity (Lumley 2010).

Earlier laboratory experiments have showed that mere presence of an experimenter is a cue that the individual should choose a behavioral strategy that is

socially acceptable (Yamagishi and Suzuki 2010). Unfortunately, it was impossible to conduct the experiment in total anonymity because participants without prior experience with computers often asked for assistance. Although social desirability bias introduced by presence of the experimenter cannot be fully ruled out, the “desired” answers were not obvious because the questionnaire asked the same questions about five different ethnic groups, not just Maharashtrians and Muslims. As can be seen from the questionnaire in Appendix B, the two key questions about discrimination against Muslims appeared among a number of similar questions about other ethnic groups in the middle of a questionnaire containing 84 survey questions. Moreover, self-administered surveys, such as through a computer interface, are characterized by a smaller social desirability bias than face-to-face interviews (Groves 2004). What is important, any bias would affect all treatment groups equally because I was present in our makeshift laboratories at all time.

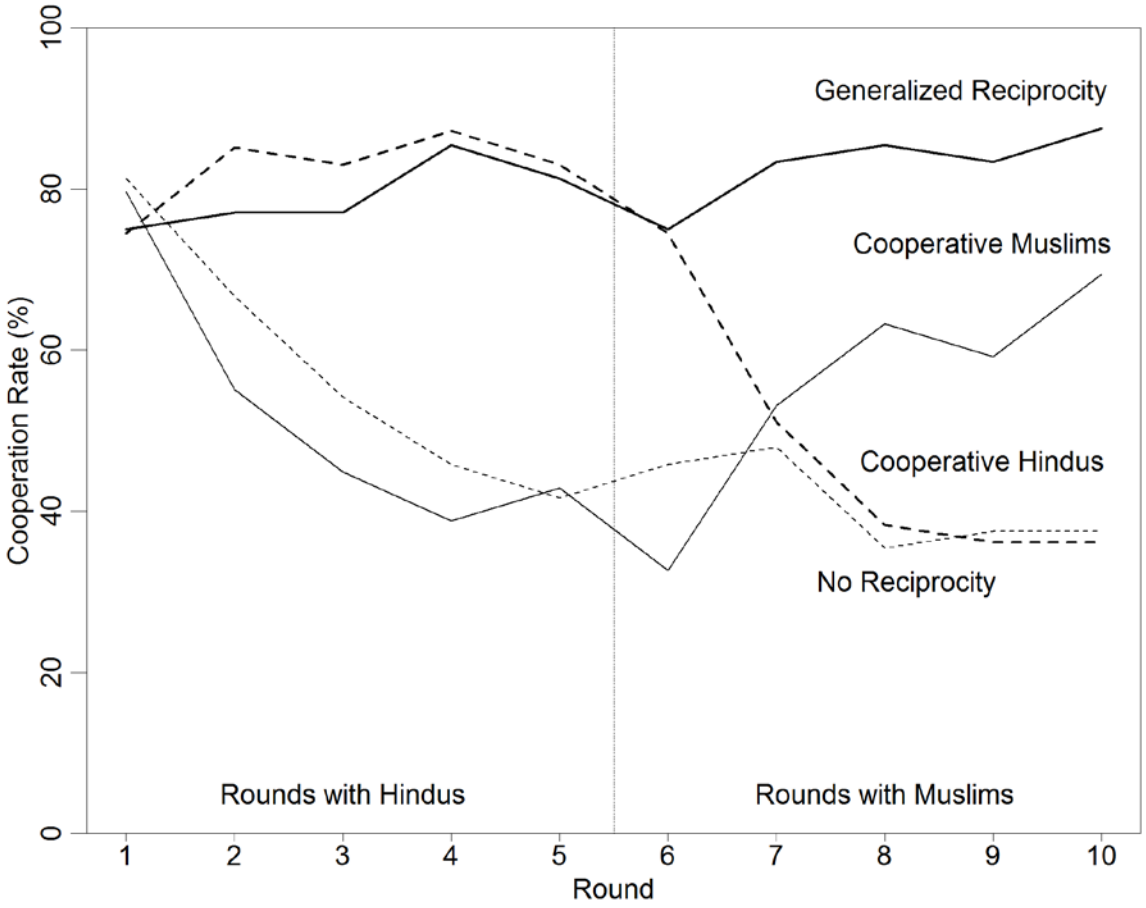
3.2 Results

3.2.1 Cooperation

Figure 1 shows that subjects understood the logic of the public goods game and updated their expectations of cooperation based on experience in previous rounds. In the first round, about 77% of participants invested real money in a common project with a stranger on a computer screen. In comparison, the average contribution in the first round in Western laboratories is typically between 40 and 60 percent of the endowment (Ostrom 2000). That said, most public goods games in Western laboratories follow the prisoner’s

dilemma payoff structure, where the best strategy is to defect 100% of time. The game used in the slums had a mixed strategy equilibrium of cooperating in 50% rounds and defecting in 50% rounds.

Figure 1: Cooperation Rate in the Public Goods Game



In just five rounds, trajectories of the four treatment groups quickly diverged. By the fifth round, the cooperation rate in the two groups that encountered Hindu defectors (Cooperative Muslims and No Reciprocity) dropped by half, in a 0.95 movement in standard deviation of the initial cooperation rate. On the other hand, the two groups that

faced cooperative players (Generalized Reciprocity and Cooperative Hindus) retained the initial high level of cooperation. In fact, the proportion of cooperators increased from 75% to 82% (or 0.17 movement in standard deviation of the initial cooperation rate). The difference of 40 percentage points between the groups with cooperative and uncooperative partners was highly significant, with all p-values lower than 0.001.[14]

In this anonymous setting, natural – as opposed to induced by social sanctioning – preference for one's own ethnic group would have manifested itself by a drop in cooperation, when ethnicity of the partner changed suddenly in the middle of the experiment from the last Hindu partner in the fifth round to the first Muslim partner in the sixth round. In reality, the drop in the cooperation rate was rather small: 6 percentage point in the Generalized Reciprocity group, 9 percentage points in the Cooperative Hindus group, and 10 percentage points in the Cooperative Muslims group. In the No Reciprocity treatment group, the cooperation rate in fact increased by 4 percentage points. None of these differences was statistically significant. Clearly, cooperation across ethnic lines in the slums of Mumbai is possible.

Willingness to invest money in a transaction with a Muslim during a laboratory game reflects the level of market integration in Mumbai. Autorickshaws, the ubiquitous three-wheelers of South Asian roads, offer a cogent illustration of the existing Hindu-Muslim economic ties. Autorickshaws in Mumbai are almost exclusively owned by Marathas. However, Marathi owners rent them out for exorbitant amounts of money to

[14] P-values in all comparisons of means across treatment groups in this study were obtained using randomization inference under the sharp null hypothesis of no difference between the two groups. Unlike a t-test or other parametric tests, randomization inference does not assume normality and independence. The only assumption is that assignment of treatment is random – which it is. Fisher (1935) developed randomization inference as the ideal nonparametric test for experimental data. An important advantage is that randomization inference will work with any sample size and any scale of the outcome variable (Gerber and Green 2012). As the theoretical argument predicts the direction of the effects, p-values are one-tailed.

Muslims and immigrants from North India, who actually drive the vehicles. As paradoxical as it sounds, the same Marathi owners typically support the political parties promoting hatred of Muslims (Shiv Sena) and of North Indian immigrants (MNS). Although a principally exploitative relationship between owners and drivers does not necessarily improve intergroup relations, familiarity with intergroup economic ties may explain the lack of ethnic bias in the public goods game. This example is in line with a string of earlier observations that, unlike indirect exchange between groups, intergroup contact in work situations does not lower prejudice (Amir 1969; Molm et al. 2003).

The cooperation rate in the Generalized Reciprocity group rose ultimately to 88 percent (0.29 movement in standard deviation of the initial cooperation rate in the sixth round). On the other hand, the proportion of cooperators in the Cooperative Hindus group dropped from 75 to 36 percent after the players realized that their Muslim partners did not reciprocate cooperation. This drop corresponds to a 0.87 movement of standard deviation.

Interestingly, the equilibrium cooperation rate in both groups that encountered Muslim defectors (Cooperative Hindus, and No Reciprocity) was the same despite a different history of the first five rounds. One third of the sample kept contributing despite their partners' defections and regardless of the defectors' social identity. Unlike conditional cooperators in the rest of the sample, they were probably motivated by altruism (Ledyard 1995), confusion (Andreoni 1995), or ambiguity aversion (Di Mauro and Castro 2011) instead of reciprocity. The number of altruistic and confused players in Indian slums is no different from that among students in developed countries. In Western laboratories, almost 30% of subjects contribute in the announced last round of a finitely

repeated prisoner's dilemma game (Ostrom 2000). In addition, approximately one tenth of the Mumbai sample defected in the last period of the game despite facing nine cooperators in previous rounds. That leaves the number of conditional cooperators in the slums just slightly higher than 50 percent. A seminal laboratory study among Swiss students by Fischbacher et al. (2001) also classified 50% subjects as conditional cooperators (and 30% as free riders).

The cooperation rate in the Cooperative Muslims group eventually doubled by the tenth round (from 33 to 69 percent). This increase by 47 percentage points corresponds to a 0.78 move in standard deviation of the cooperation rate in the sixth round. However, the Cooperative Muslims group never converged at the same high level of cooperation as the Generalized Reciprocity group (88%, difference with a p-value of 0.02). A pairwise comparison of means shows that the average final contributions in the Generalized Reciprocity and Cooperative Muslims groups are significantly different from those in the Cooperative Hindus and the No Reciprocity groups (all p-values<0.001). There is no statistically significant difference between the average final contributions in the Cooperative Hindus and No Reciprocity groups.

Qualitative data gathered during interviews after the experiment suggest that players were not as risk averse as I expected. This is how one member of the ill-fated No Reciprocity group explained his strategy: "I went on with the investment of ten Rupees every time in order to get the maximum profit. My partners didn't invest more than zero Rupees and I ultimately ended in gaining no profit at all." What is intriguing, some subjects kept investing despite no contributions from their partners. As one of them said: "My partner didn't invest but I didn't feel anything for that. Still I went on investing the

amount.” These dispassionate responses suggest that my study may have a problem with the house money effect. Building on prospect theory, Thaler and Johnson (1990, 657) argued that experiments with financial incentives often suffer from the same problem: “After a gain, subsequent losses that are smaller than the original gain' can be integrated with the prior gain, mitigating the influence of loss aversion and facilitating risk-seeking.” As the subjects in the slums were playing with the money given to them by the experimenter, they were more willing to take risks than if they had to earn the same amount of money by working. Risk-seekers probably cooperate more even if they should not (i.e., when they expect the other player to defect). As a result, the difference between people with high and low expectations of cooperation may be even bigger than it appears.

Conditioned by past behavior, ethnically heterogeneous pairs produced as much public goods as ethnically homogenous pairs in the public goods game. Regardless of ethnicity, participants cooperated with those whom they trusted to reciprocate cooperation. But did a positive interaction with Muslims change attitudes towards Muslims as a social category?

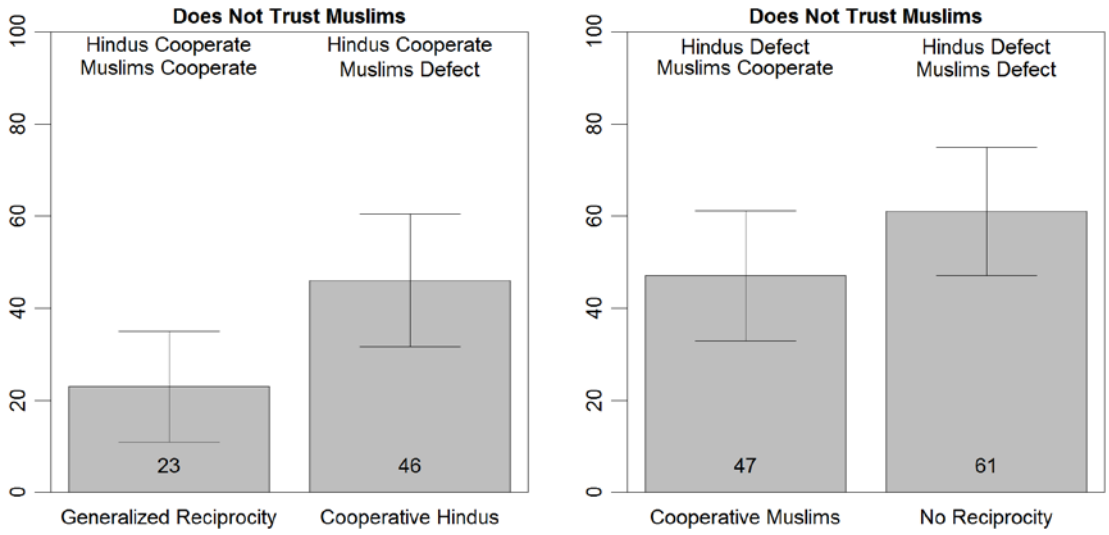
3.2.2 Trust

This dissertation advances an argument that experiencing indirect reciprocity in interactions with individual Muslims should change stereotypical beliefs about cooperative behavior of Muslims as a group. If this is true, we should observe increased generalized trust of Muslims in the treatment groups that interacted with cooperative Muslim players. In order to identify the average treatment effect, this and the subsequent

section compare only the treatment groups that shared the same history of play with ingroup members and differed in nothing but cooperativeness of outgroup members: Generalized Reciprocity vs. Cooperative Hindus, and Cooperative Muslims vs. No Reciprocity. Comparing all four groups together would not allow to disentangle the treatment effect of the rounds played with Muslims from the effect of the rounds played with Hindus.

Figure 2 shows the percentage of subjects who reported in the post-treatment survey that they had “no trust at all” or “not very much trust” in Muslims. Interaction with cooperative outgroup members reduced distrust of the outgroup as a whole by 23 and 15 percentage points, with p-values of 0.02 and 0.08 respectively, in comparison to the corresponding treatment groups that faced noncooperative Muslims. Table A.2 in the Appendix shows that these results were practically unchanged by covariate adjustment.

Figure 2: Distrust of Muslims by Treatment Group, percentage with 95% Confidence Intervals



Although the experiment was ostensibly about “cooperation between people from different neighborhoods” and never mentioned “Muslims” as a group at all, subjects generalized their experience with individual seemingly Muslim partners on the computer screen to the Muslims as a social category and updated their stereotypes.

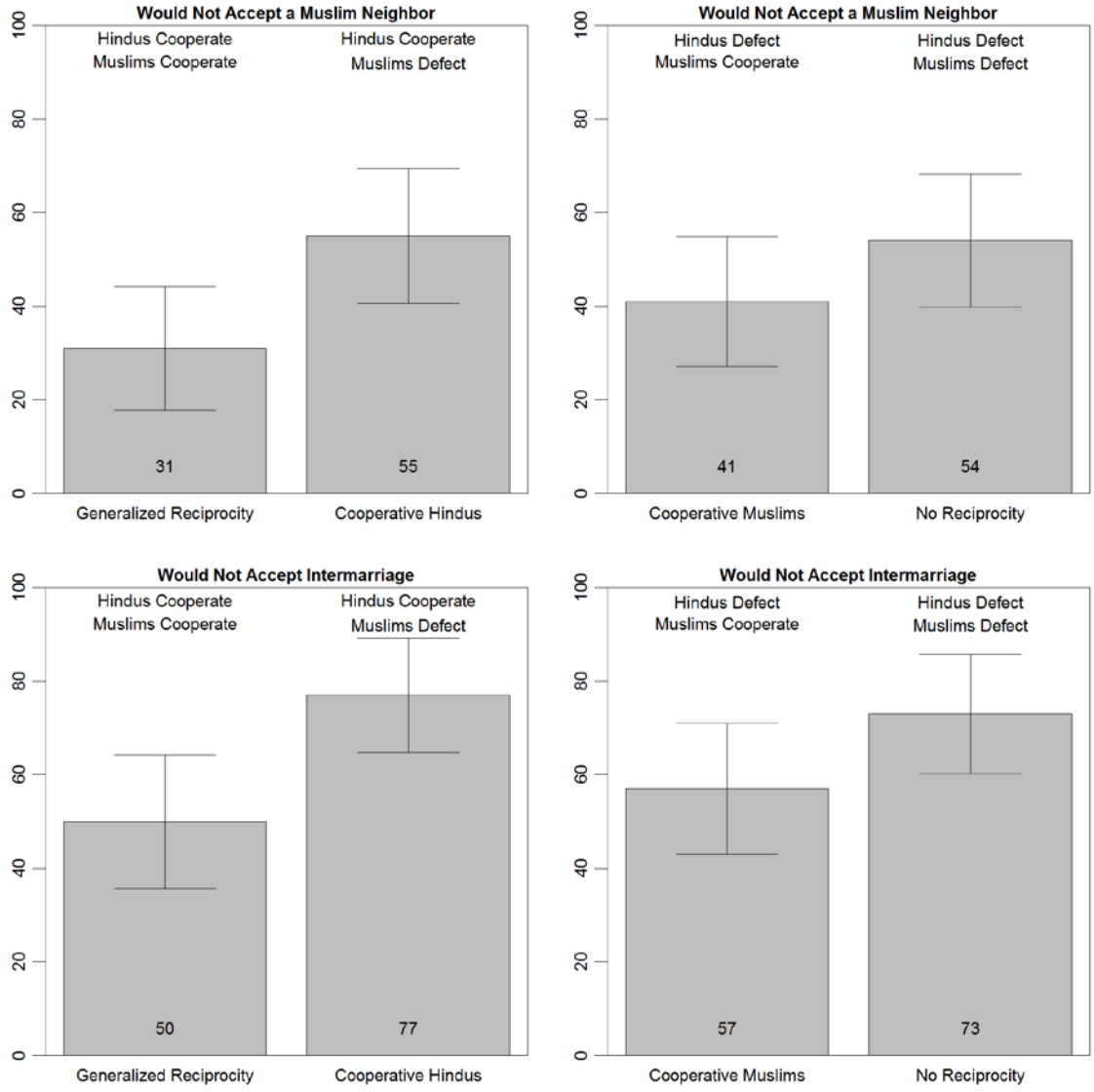
When asked later to explain why they trusted Muslims, five subjects mentioned positive reciprocity. In the words of one of them: “I trust the Muslim people because we have Muslims as our neighbors. They help us in all situations and they also mix with us. They do not try to hurt us.” Three subjects justified their mistrust of Muslims by negative reciprocity, believing that Muslims do try to hurt members of other groups and keep social distance by not inviting Hindus to their houses. Three subjects perceived Muslims as a heterogeneous mix of “good and bad people,” while two subjects talked about the shared history and identity.

3.2.3 Social Distance

The post-treatment survey shows that interaction with Muslims during the experiment changed discriminatory attitudes towards Muslims as a group. Let us look first at the survey question whether the respondent would accept a Muslim as a neighbor (top two graphs in Figure 3). The proportion of people who would not accept a Muslim neighbor dropped from 54% in the No Reciprocity condition to 41% in the Cooperative Muslims group. Although in the predicted direction, the treatment effect was not statistically significant in this case (p-value=0.09). On the other hand, the difference of 24 percentage points between the Generalized Reciprocity and Cooperative Hindus groups

was significant (p-value=0.01, 0.5 standard deviation movement). As these two groups differed only in whether their Muslim partners cooperated with them, the large effect can be attributed to the treatment and not to pre-existing differences between the two groups.

Figure 3: Avoidance of Muslims by Treatment Group, percentage with 95% Confidence Intervals



The effect of reciprocity remains strong even after controlling for covariates (Table 5). As covariate adjustment allows to estimate the treatment effect more precisely, both coefficients are now statistically significant.

Table 5: Logistic Regression Predicting Social Distance (Neighbors)

	Generalized Reciprocity and Cooperative Hindus groups		Cooperative Muslims and No Reciprocity groups	
Model	(1)	(2)	(3)	(4)
Generalized Reciprocity	-1.002*	-1.399**		
	(0.429)	(0.523)		
Cooperative Muslims			-0.539	-1.114*
			(0.411)	(0.526)
Magathane		0.435		1.346.
		(0.663)		(0.743)
Shivaji Nagar		0.719		1.037
		(0.740)		(0.899)
Bhendi Bazar		-1.106*		-0.151
		(0.539)		(0.509)
Hindu		-0.065		-0.088
		(0.650)		(0.636)
Age		0.006		0.040
		(0.034)		(0.031)
Education		0.011		0.171.
		(0.052)		(0.094)
Income		-0.002		0.008
		(0.007)		(0.007)
Extremist Voter		0.601		-0.943.
		(0.572)		(0.508)
Intercept	0.214	0.247	0.167	0.360
N	95	88	97	89

Design-based robust standard errors in parentheses.
. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

The post-riots Mumbai is an ethnically segregated society. 149 out of 210 respondents (71%) in the control group said they would not accept a Muslim neighbor,

while only 3 out of 210 respondents would not accept a Marathi. Although experimenters rarely ask subjects about their inner motivations, I was interested in understanding why subjects answered this question as they did. The qualitative data support the story that arose from the quantitative analysis. Only one of eight interviewed respondents did not mind Muslims living in his neighborhood. His explanation nicely illustrates the causal argument advanced by this study: "We allow the Muslim people to be our neighbors because they are very *honest* and *help us* all the time in all the situations" (italics added by author for emphasis). In other words, the subject was motivated by positive reciprocity. Seven respondents offered a variety of explanations of why they did not desire Muslims in their neighborhood. According to one respondent, Muslims "are not good people," "cannot behave nicely," and "always quarrel with their neighbors." Another said that Muslims "are not good people by nature" and "cannot behave in good manner." Another respondent even called Muslims "goons by nature." These responses reveal negative reciprocity driven by adverse stereotypes of Muslims. Two interviewed participants were afraid of a perceived threat. Among them, the most appalling, but also somewhat understandable answer in the context of the violent ethnic conflict in Mumbai, was: "I do not accept the Muslim people as my neighbors because it is very difficult to survive beside them in the time of riots." Finally, two answers highlighted cultural differences between ingroup and outgroup. According to these two respondents, "[Muslims'] living standard is not well according to our culture" and "they give too much importance to their religion." The interviews show clearly that participants had their real living conditions in slums – and not some hypothetical counterfactual of a more peaceful society – in mind while answering the survey question on social distance. Therefore, the

qualitative evidence increases confidence in internal validity of the experimental results.

Surprisingly, the change in prejudice could be discerned easily even from the answers to Bogardus' harshest indicator of social distance, that is the question whether the participants would “accept a Muslim to close kinship by marriage.” In India, marriage is one of the most revered institutions. According to a survey from 2005, 95% of Indian marriages are arranged by parents with little to no say of the actual bride (Desai and Andrist 2010). Predictably, a vast majority of marriages occur not only within the same religion, but also within the same caste (*jati*). The control group displayed a strong preference for ethnic endogamy: 170 out of 210 respondents (81%) said that they would not accept a Muslim marrying into their family, while 209 out of 210 respondents would accept a Maharashtrian.

Given the importance of ethnicity in the selection of a marriage partner, I did not expect any effect of the experimental treatment on attitudes concerning intermarriage. Nevertheless, as the bottom portion of Figure 3 shows, the experimental treatments decreased the number of people who would not accept a Muslim to marry into their family by 27 (from Cooperative Hindus to Generalized Reciprocity) and 17 (from No Reciprocity to Cooperative Muslims) percentage points. The difference-in-means estimator of the ATE is fairly large – a 0.4 to 0.6 movement in standard deviation – and statistically significant (with a p-value of less than 0.01 for the first comparison and less than 0.05 for the second one). After including all the control variables in the regression, the treatment effect of Generalized Reciprocity retains its statistical significance (p-value < 0.001), but the effect of Cooperative Muslims is significant only at the 0.1 level (see Table 6).

Table 6: Logistic Regression Predicting Social Distance (Intermarriage)

	Generalized Reciprocity and Cooperative Hindus groups		Cooperative Muslims and No Reciprocity groups	
Model	(1)	(2)	(3)	(4)
Generalized Reciprocity	-1.186**	-2.208***		
	(0.448)	(0.638)		
Cooperative Muslims			-0.703	-0.917.
			(0.436)	(0.544)
Magathane		-0.928		1.272*
		(0.877)		(0.628)
Shivaji Nagar		-1.850.		1.732.
		(0.944)		(0.887)
Bhendi Bazar		-1.698**		-0.249
		(0.560)		(0.556)
Hindu		1.049		0.609
		(0.841)		(0.627)
Age		0.055		-0.033
		(0.039)		(0.031)
Education		-0.117.		0.009
		(0.060)		(0.065)
Income		0.006		0.007
		(0.007)		(0.006)
Extremist Voter		0.104		-0.153
		(0.645)		(0.531)
Intercept	1.186***	2.533**	0.990**	-0.160
N	95	88	97	89

Design-based robust standard errors in parentheses.
. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

I suspect that the treatment effect appears to be so strong because it was measured immediately after the experiment. I could not observe how quickly the effect of reciprocity decays in time. This is a general weakness of all laboratory, lab-in-the-field, and survey experiments. One can imagine a follow-up study, in which measurement of hostile behavioral intentions – or of behavior itself – occurs in various randomized intervals ranging from hours to months after the experiment. Since such a modified

research design would make a debriefing more difficult, it would prevent the use of deception. It would also raise ethical concerns about artificially induced negative stereotypes influencing subjects outside the laboratory.

By necessity, the treatment groups used for pairwise comparisons differed not only in whether Muslim partners cooperated, but also in the overall payout gained during the experiment. For example, as subjects in the Generalized Reciprocity group never faced defectors, they earned more money on average than subjects in the Cooperative Hindus group. Higher earnings may have led to less discrimination simply due to improved mood or some other reason. Unfortunately, as payout and treatment assignment are by necessity highly correlated (with r equal to 0.80 for the Generalized Reciprocity and Cooperative Hindus groups), multicollinearity prevents inclusion of the payout in the same regression as the treatment.

What I could do instead was to compare two treatment groups with the same number of cooperative partners (Cooperative Hindus vs. Cooperative Muslims). Despite similar earnings (Rs. 130 in the Cooperative Hindus group and Rs. 115 in the Cooperative Muslims group), discrimination in the Cooperative Muslims group was lower by 19 percentage points (p-value=0.03) using the question about intermarriage and by 15 percentage points for the question about a Muslim neighbor (p-value=0.11).

The questionnaire asked the same questions about four different outgroups: Muslims, Gujaratis, Biharis, and Parsis. If discrimination against Muslims is lowered by higher earnings and not group-specific reciprocity, we should observe the same effect on discriminatory attitudes towards other outgroups as well. Figure A.1 in the Appendix plots average discriminatory attitudes in four treatment groups. Whereas patterns of

discrimination against Muslims are in the predicted direction (thick lines, with lower discrimination in the Generalized Reciprocity and Cooperative Muslims groups), discrimination of other outgroups (thin lines) does not follow any discernible pattern, except for perhaps slightly higher discrimination in the No Reciprocity group. If driven by earnings, all lines, whether thick or thin, should correlate with the average payout – represented by the number of Rupees just above the x-axis.

The treatment in the public goods experiment had a negative effect on discrimination, as predicted by the theory, across a variety of specifications (comparisons of means, regressions, two different measures of social distance). Subjects generalized their positive experience with Muslim cooperators to the ethnic group as a whole. At the same time, negative experience with Muslims during the experiment did not increase discrimination because avoidance of Muslims in the Cooperative Hindus and No Reciprocity groups was not significantly higher than in the control group (see Table A.3 in the Appendix).

3.2.4 Subgroups

The findings would be less relevant if I found out that, for example, political extremists were immune to the treatment. Therefore, I replicated the regression analysis, but this time including interactions between treatments and two covariates of interest: religious affiliation and past voting for extremist parties. As Table A.4 in the Appendix shows, interaction terms were statistically insignificant. Moreover, when I replicated all the regressions using a sub-sample restricted to Hindus, it did not change any of the

substantive findings – only made some of the treatment coefficients appear slightly more or less significant than before. In other words, Hindus responded to increased expectations of cooperative behavior in the same way as Buddhists did and voters of extreme nationalist parties (Shiv Sena and MNS) in the same way as moderates. A homogenous effect across different subgroups gives more confidence in the external validity of the results.[15] The fact that supporters of extremist political parties responded to the treatment equally well as the moderates has probably even more encouraging policy implications.

3.2.5 Ethnocentrism

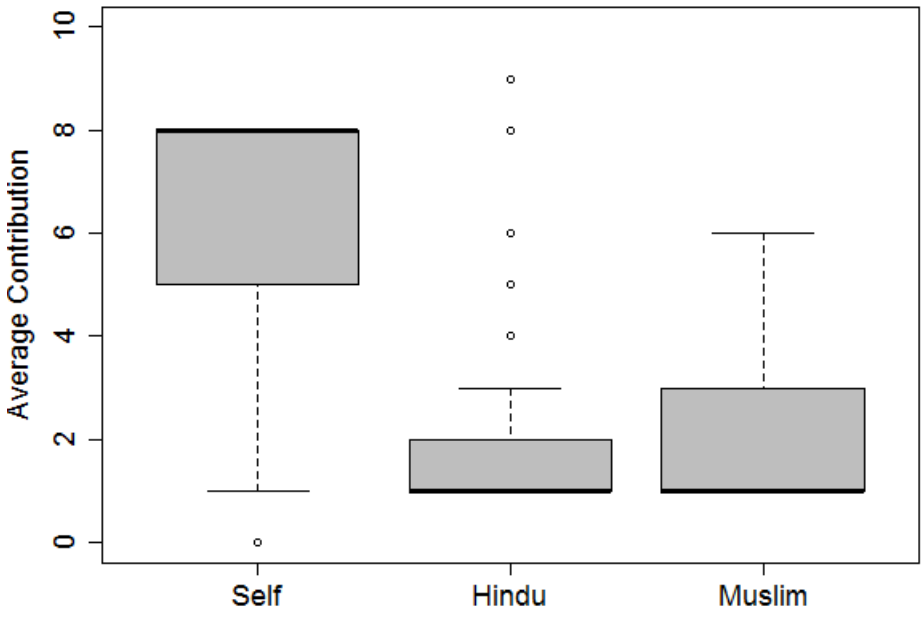
The experimental intervention reduced the pre-existing ethnic bias substantially, but it did not erase it completely. Although acceptance of Muslims as potential neighbors was higher in the Generalized Reciprocity (69%) and Cooperative Muslims (59%) groups than in the control group (29%), it was much lower than acceptance of fellow Maharashtrians (92-99%, depending on the treatment group). Similarly, the Generalized Reciprocity group trusted Muslims (77%) more than the control group did (59%), but ingroup members still enjoyed more trust across all treatment conditions (83-96%).

Curiously, Hindu subjects in the slums of Mumbai displayed little ethnic favoritism in laboratory games. I already discussed likely reasons for why the cooperation rate in the public goods game did not change much as ethnicity of partners

[15] The treatment effect may still be heterogeneous across some unobserved subgroups. However, this heterogeneity does not necessarily undermine external validity of the findings. According to Monte Carlo simulations run by Druckman and Kam (2011), the estimate of the treatment effect will be biased only if the unobserved variable that is moderating it does not vary virtually at all in the sample.

suddenly changed between the fifth and sixth round. Similarly, Figure 4 demonstrates that the anonymous dictator game did not reveal any natural ingroup bias in how subjects distributed money between themselves, a Hindu, and a Muslim. The difference between the amount of money allocated on average to an ingroup member (Rs. 1.79) and to an outgroup member (Rs. 1.86) was insignificant both substantively and statistically (p-value=0.457). In fact, the modal strategy was to give each of the two other players one Rupee and pocket the rest. When not observed by anyone, 52% of participants rewarded themselves with 8 out of 10 Rupees that they could distribute. Ethnocentrism was not a strong factor in people's decisions during the experiment – especially while compared to the strong causal effect of reciprocity.

Figure 4: Average Contributions in the First Dictator Game



This result contradicts the usual understanding of social identity theory and some

earlier observations. For example, Democrats and Republicans in the United States gave more to the recipient from their own party than from the other party while playing the dictator game (Fowler and Kam 2006). On the other hand, Yamagishi and Mifune (2008) reported that ingroup favoritism in the dictator game disappeared in an anonymous setting, when recipients did not know the identity of the dictator. Similarly, Habyarimana et al. (2009) found that subjects in Uganda did not favor their coethnics, when they were free to express their true preferences in an anonymous setting. The dictator game in India confirms these earlier findings from Japan and Uganda. After anonymity disentangled altruism from the effect of social sanctioning, we observe no ethnocentric altruism and, in effect, not much general altruism either.

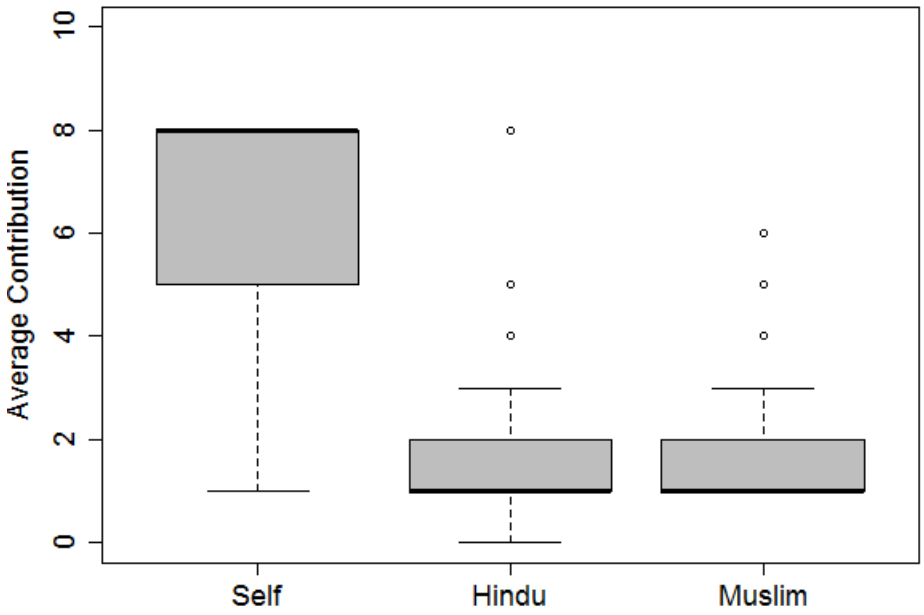
Qualitative evidence from a small number of interviews conducted after the experiment confirms a limited role of ethnocentrism. None of the eleven subjects asked to explain their motivation in the first dictator game mentioned ethnicity. In fact, they were mostly talking about themselves. A typical answer was “I kept the money with me because I wanted more money.” Several interviews suggested that altruism in the slums of Mumbai does not extend beyond one’s immediate social network and therefore may not be generalized to the imagined communities of big ethnic groups. As one subject put it: “I gave one Rupee to each of them because I didn't know them at all.”

Although the experiment did not manipulate altruism, observational data collected during the experiment provide more evidence for insignificance of this variable. Table A.5 in the Appendix shows that baseline altruism towards Muslims, as revealed by the dictator game played before the experimental manipulation, did not correlate with discriminatory attitudes expressed in the post-treatment survey. Reciprocity and altruism

are two very different aspects of other-regarding preferences (Meier 2006). In Indian slums, it seems, there is not much altruism at the first place and the existing altruism does not predict discrimination very well. Reciprocity, on the other hand, has a strong causal effect on discriminatory attitudes.

The effect of reciprocity is direct and independent from altruism. Comparing the results of the dictator games played before and after the treatment shows that a positive experience with Muslims reduced discriminatory intentions without changing the baseline altruism. Table A.6 in the Appendix regresses the difference between the amount given to a Muslim in the second dictator game and the amount given to a Muslims in the first dictator game on treatment. The treatment effect is indistinguishable from zero and strategies used in the two dictator games did not differ much (see Figure 5).

Figure 5: Average Contributions in the Second Dictator Game



Social identity theory and its extensions in political science predict that people with a salient collective identity should favor coethnics more and they should discriminate more strongly against the outgroup (Brown 2000). As Brewer (1999) discusses at length, these two predictions are often conflated in the literature, though negative attitudes towards outgroups are not necessarily related to ingroup bias. There is little evidence for either claim in the observational data gathered during the Mumbai experiment. To find out more about the statistical association between identity salience, ingroup bias, and outgroup discrimination, the questionnaire included two covariates that serve as proxies for ingroup identity salience: attachment to Maharashtra (and thus to the Marathi identity) and frequency of praying (indicating the person's attachment to the Hindu religion). "Attachment" to one's identity is considered to be the best measure of identity salience (Sinnott 2006) and the wording of my question is used on a regular basis by Eurobarometer. For the model with the frequency of prayer, I included only the Hindu subjects.

Table A.7 in the Appendix summarizes the results for discriminatory attitudes. Religious Hindus are no more or less likely to tolerate Muslims, though the question about accepting a Muslim neighbor may indicate slightly stronger discriminatory intentions among more religious people (p-value=0.06). A Salient Marathi identity turned out to be a poor predictor of acceptance of a Muslim marrying into the subject's family. In another surprising result, attachment to Maharashtra correlates negatively – not positively – with discrimination against Muslim neighbors (p-value=0.03).

Table A.8 tests the weaker claim that people with a stronger salient ingroup identity favor their coethnics more. In reality, there is no statistical association between

identity salience and ingroup favoritism, as measured by the amount of money given to a Hindu in the dictator game.

The results of the Dictator Game contradict Hamilton's Rule, according to which people aid their genetic relatives more than strangers (Hamilton 1996). Given the low level of intermarriage between Maharashtrians and Muslims, there should be more genetic variation between these two groups than between the Maharashtrian subjects and their supposedly Maharashtrian partners. However, Marathi facial features and names did not elicit more altruism than photographs and names of Muslims. Despite contradicting a popular socio-biological view, this finding is consistent with an empirical observation from a very different part of the world. Analyzing survey data from the United States, Phan et al. (2009) found that Americans rely on friends, neighbors, and co-workers more than on siblings or other relatives, with the exception of parents and children.

3.2.6 Reciprocity and Contact

Chapter 1 suggested that reciprocity is a causal channel through which intergroup contact reduces prejudice. Although we will examine the connection between contact, reciprocity, and prejudice more fully in the next chapter, observational evidence in the experimental data indicates that the detected treatment effect can be attributed to reciprocity during the experiment and not to previous intergroup contact. Coefficients of the treatment variables did not change when the covariate adjustment included an indicator of acquaintances in the Muslim enclave of Bhendi Bazar (Tables 4 and 5). The treatment effect remained strong even after controlling directly for quantity and quality of

previous contact with Muslims (Table A.9 in the appendix).[16]

Table A.9 also shows that people who had been taken advantage of by Muslims were no more or less responsive to the updating of their stereotypes during the experiment. Nor was there any sign of a stronger (or weaker) treatment effect among those who had been in less frequent contact with Muslims. Statistical insignificance of the interaction terms included in the models indicates that previous intergroup contact did not moderate the treatment effect.

Finally, let us look at the optimal conditions of positive contact proposed by the contact hypothesis literature. Three of the optimal conditions of positive contact (common goal, cooperative interdependence, support of the authorities) were held constant at a positive value across all four treatments. Both the subject and his on-screen partner shared the common goal of earning money, depended on each other in this pursuit, and the experimenter sanctioned their positive interaction. Due to a lack of variation, these three conditions cannot explain the detected differences between the treatment groups.

The fourth condition, equal status, could not be met during the interaction because the social status of Muslims in Mumbai is considered generally lower than the social status of Maharashtrians. On average, the sampled slum-dwellers also expressed this belief. In the post-treatment survey, participants were asked to place various ethnic groups living in Mumbai each on one of ten rungs of a hypothetical ladder corresponding

[16] Quantity of intergroup contact is measured by the answer to the question "How often do you have informal talks with Muslims?" running from -2 ("never") to 2 ("very often"). Quality of contact is measured by the question "How often have you been taken advantage of by a Muslim?" using the same scale. These measures come from Islam and Hewstone (1993). Their advantage is that they have been used in the population of Indian Hindus and Muslims before, though only in a very small student convenience sample (Tausch et al. 2009).

to socioeconomic status.[17] The subjects perceived other Marathi people on average as having a significantly (p-value<0.001) higher social status (5.9 out of 10) than Muslims (4.4 out of 10).

Since some individuals might have perceived Muslims as more or less equal in social status to the Marathi people, I also regressed discriminatory intentions on the perceived difference in social status between these two groups. The coefficient of relative social status in the regression predicting social distance was close to zero and statistically insignificant (see Table A.10 in the Appendix). As all interactions between social status and treatments were insignificant as well, the social status differential did not moderate the causal effect of reciprocity. The Mumbai experiment shows that it is possible to reduce prejudice by a very specific type of positive intergroup contact even in the case of unequal social status.

3.3 Conclusion

Experiencing indirect reciprocity in a laboratory game increased trust and reduced discriminatory intentions in two completely unrelated domains – ethnic segregation of housing in slums and intermarriage. This finding indicates that expectations of reciprocity are not only generalizable from the individual to the group, but also across situations. The treatment produced a robust causal effect in the predicted direction on cooperation, trust,

[17] The MacArthur Scale of Subjective Social Status that I used was originally developed to measure subjective social status in health surveys (Goodman et al. 2001). To my knowledge, it has not been employed in political science yet despite its growing popularity in other fields. Post-treatment measurement of covariates should be generally avoided because such variables may be in reality endogenous to the experimental intervention itself. However, the treatment most likely did not affect the perceived social status of Maharashtrians and Muslims because there are no significant differences in the average social status of these two ethnic groups across the four treatment groups.

and discrimination across different specifications of the model. The treatment effect seems to be homogenous across various subgroups. At the same time, salience of social identities, altruism, and previous intergroup contact explain surprisingly little variation of the dependent variables.

The optimistic conclusion of this chapter naturally leads to the question why we see so much ingroup bias and so little outgroup bias in real life. One possible explanation is that it is easier to find and punish someone who exploited one's trust if the person belongs to one's own group (Habyarimana et al. 2007). In addition to this straightforward mechanism, information about the person's untrustworthiness can spread through her social network and reach potential partners that have not encountered her yet. Since social networks are usually denser within than across ethnic groups, it should be easier to obtain information about the past of a coethnic (Fearon and Laitin 1996). Laboratory experiments in different countries detected more cooperation between coethnics only when they were seen by others to cooperate, and no preference for ingroup members in an anonymous setting, in which no punishment of defectors was possible (Habyarimana et al. 2009; Yamagishi and Mifune 2008). If social sanctioning generally facilitates cooperation with ingroup members, it will increase expectations of reciprocity within the ingroup in the long run. In terms of my model, this tendency will lead to a situation equivalent to the Cooperative Hindus treatment group.

Chapter 4. Selective Discrimination of Outgroups

Laboratory experiments described in the previous chapter identified a clear causal relationship between reciprocity and discrimination against the Muslim minority. This chapter uses survey data collected in the control group to assess whether we can observe the same association between real-life reciprocity and discrimination in relations to a larger number of ethnic minorities. The two chapters complement each other. The experiment allows to draw causal inference unbiased by endogeneity. The survey shows that the identified relationship between the key variables is not an artifact of the laboratory setting or some particular Hindu-Muslim dynamics. The survey analysis presented in this chapter also provides an explanation for why the same person often discriminates against some, but not other outgroups.

4.1 Puzzling Selective Discrimination and Its Explanations

Chapter 1 discussed briefly the puzzling selective discrimination of outgroups. Why would the same person discriminate against a Russian immigrant, but not against an Irish immigrant? Why would a Marathi rioter kill a Muslims neighbor and spare a Gujarati? The theory proposed in this dissertation makes a prediction about the likely patterns of discrimination across multiple outgroups. If people tend to discriminate against groups not believed to be cooperative – as the experiment in Chapter 3 demonstrated – we should be able to observe more discrimination targeting mistrusted ethnic groups in real life. Furthermore, if people update their stereotypes as theorized in

Chapter 1, we should be able to observe a negative correlation between a person's discrimination against a particular group in the present and cooperative behavior by members of the same group experienced by the same person in the past.

Obviously, this correlation may be spurious if not controlled for previous intergroup contact. Contact itself plausibly increases the number of opportunities for positive (and negative) experience. However, contact's effect on discrimination may be mediated by other factors apart from reciprocity. Pettigrew and Tropp (2006) listed increased liking, reduced anxiety, perceived outgroup variability, and several other candidates for intervening variables between intergroup contact and prejudice. Therefore, the direct effect of intergroup contact, or, rather, its effect unmediated by positive reciprocity is an alternative explanation that one has to take into account.

To assess relative importance of reciprocity, this variable should be also compared to other existing explanations. According to a view associated with realistic group conflict theory, competition for scarce resources drives intergroup conflict (Sherif et al. 1961). Obviously, not all groups compete for scarce resources. Posner (2004) predicted that relations should be worse between groups that are large enough to form a minimum winning coalition required to seize control of the contested resource. Consequently, a dominant group, such as Marathi-speakers, should feel threatened more as the relative size of an ethnic minority increases (Blalock 1967).

Although relative group size is a plausible objective predictor of realistic conflict between groups, perception of collective threat is ultimately subjective. In principle, people can be concerned with perceived group threat in three major domains. First, an ethnic minority can be regarded as either a direct economic competitor or a scapegoat for

worsening economic conditions (Blalock 1967; Quillian 1995; Hardin 1995). Second, it can represent a threat to the person's safety (Sniderman et al. 2004) due to the group's association with crime, terrorism, or open warfare. Third, the minority can pose a threat to cultural identity (Sniderman et al. 2004). These types of threat are to some degree overlapping with the classification proposed by Stephan and Stephan (2000). In their integrated threat theory of intergroup attitudes, Stephan and Stephan differentiated between "realistic threats" to the group's political and economic power and "symbolic threats," which are essentially perceived differences in norms and values.

Another factor to take into account is relative status. Empirical research in psychology reviewed by Hewstone et al. (2002) and Brown (2000) shows that high-status groups express more group bias than low-status groups. Their bias is especially strong if they perceive the status gap to be closing. Mirroring these laboratory results, Petersen (2002) found that status reversals triggered ethnic violence in twentieth century Eastern Europe. For example, Ukrainians attacked the resented Polish settlers in 1939, after the Soviet occupation reversed abruptly the high status of the Polish ethnic identity. In his book about Russian minorities in the "near abroad," Laitin (1998) argued that the high social status of westernized Baltic nations may persuade the Russian minority in Latvia to assimilate, while Russians living in Kazakhstan are reluctant to adopt the language of the low-status local majority. In Laitin's account, social status of ethnic groups in the post-Soviet space is derived from their perceived level of modernity. There is also some evidence suggesting that the concept of social status obscures several different dimensions, each of them producing different behavior. For example, Cuddy et al. (2007) found that both admired and pitied groups elicited helping behavior, while envied and

hated groups elicited active harm.

Finally, the last group-level explanation to test my theory against is the common ingroup identity model, formulated by Gaertner et al. (1989) and Gaertner et al. (1993). It claims that people discriminate less against the outgroups that share some superordinate identity with them. A superordinate identity is an identity held by the members of otherwise distinct subgroups, along with their particular subgroup (subordinate) identities. For example, Germans and French share a common European identity without surrendering their national identities.

4.2 Research design

I regressed discriminatory attitudes on various measures of reciprocity and intergroup contact while controlling for prominent alternative explanations from the literature. The dataset is based on a survey of 210 randomly sampled adult male Marathi-speaking slum-dwellers who served as a control group in the experiments described in the previous chapter. They were all interviewed by two male Hindu research assistants in the Marathi language. They did not receive any experimental treatment that could affect the results.

The questionnaire asked the Marathi respondents a number of questions about four different ethnic minorities living in Mumbai: Muslims, Gujaratis, Biharis, and Parsis. The groups were chosen to maximize variation in the independent variables.[18] As the same person could discriminate in different ways against these four ethnic groups, the

[18] Table A.11 in the Appendix summarizes the descriptive statistics of the used variables.

unit of observation is the respondent-outgroup dyad. All independent and dependent variables are measured separately for each dyad. Each respondent is thus responsible for four observations in the dataset. Since we cannot consider these observations statistically independent, robust standard errors are clustered at the level of individual respondent.

Just as in the previous chapter, social distance serves as a proxy for discriminatory attitudes. Again, I used two different questions from the social distance scale developed by Bogardus (1925). The first question asks: “If it was up to you, would you accept a [member of a group] as a neighbor?” The second question asks: “If it was up to you, would you accept a [member of a group] to close kinship by marriage?” I took the average of the two to produce a composite indicator of discrimination, running from 0 (would accept both a neighbor and a family member) to 1 (would not accept either).

The measure of intergroup contact is based on Islam and Hewstone (1993) and has been used to study Hindu-Muslim relations in India by Tausch et al. (2009). The question asked: “How often do you have informal talks with [group]?” The question was asked separately about Muslims, Gujaratis, Biharis, and Parsis. Respondents could pick an answer from a scale ranging from -2 (never) to 2 (very often).

To capture the effect of positive reciprocity, the survey asked respondents whether they had experienced cooperation with members of the listed outgroups: “In the past twelve months, which people, if any, have helped you directly by giving you money or some of their time?” A separate dichotomous variable for each group was coded 1 if the respondent had received any help from a member of that particular group and 0 otherwise. Similarly to this work, Coleman (1988) defined reciprocity by giving and receiving help. The specific indicator employed in this study is inspired by a question in

the National Survey of Families and Households (NSFH) that Phan et al. (2009) used to study reciprocity in the United States.

Self-reported received help is not a perfect indicator of the outgroup's real cooperative behavior because the respondent's perception can be biased by the quality of intergroup relationship. On the other hand, attitudes are motivated by the person's perception of cooperativeness, not an objectively measured real record of cooperation. Due to the potential endogeneity problem, the analysis presented in this chapter is not as strong as the direct experimental test of the theory in Chapter 3. Saying that, both chapters found the same hypothesized correlation between reciprocity and ethnic discrimination. In another caveat, received generosity is but one example of cooperative interaction. As people could learn about other groups' cooperativeness from many other sources, the used indicator is probably very noisy.

Increased contact does not create only opportunities for cooperation, but also for defection. That is why the analysis also includes negative reciprocity. Negative reciprocity is measured by the question "How often have you been taken advantage of by [group]?" Tausch et al. (2009) adopted this question from Stephan et al. (2002). For each outgroup, a Likert-type scale runs from -2 (never) to 2 (very often). This conceptualization differs from that used by Phan et al. (2009, 902), who used the terms "positive" and "negative" reciprocity to describe "whether an individual is under benefiting or over benefiting from their helping network, relative to the expected norm of balanced exchange." The rationale is that people can form negative stereotypes about other groups if they experience defection from their members. Again, to be taken advantage by someone is only one of many possible indicators.

Relative size of the outgroup is measured by the outgroup's proportion of the total population of Mumbai according to the Census of India.[19] Muslims and Gujaratis represent 19% of the population each, while Biharis (1.5%) and Parsis (0.5%) are much smaller minorities. Inclusion of these four ethnic groups in the questionnaire thus maximizes the variation in group size.

Chapter 2 characterized Mumbai as a city plagued by violence caused by Muslim terrorism, Hindu rioting, and organized crime. It also described fierce intergroup competition for jobs. Therefore, I included two different subjective measures of threat perception. The corresponding survey items asked whether the respondent agreed or disagreed with several statements. The statements measuring the perceived threat to the economic well-being were "[Group] take jobs away from people like me." The statements related to the threat to safety were "[Group] increase crime rates." Answers appeared on a Likert-type scale running from -2 (disagree strongly) to 2 (agree strongly). The used questions come from the ISSP National Identity Survey.

Relative status is simply the difference between the perceived social status of the outgroup and the perceived social status of Maharashtrians. The survey item asked respondents to place each group on one of ten rungs of a hypothetical ladder corresponding to socioeconomic status. The resulting MacArthur Scale of Subjective Social Status is explained in more detail in Goodman et al. (2001).

Most Gujaratis and Biharis living in Mumbai are Hindus. Muslims and Parsis are separate minority religious groups. As Gujaratis and Biharis share a common Hindu

[19] If the sampled respondents members came from more than one group, I would have to use the size of their group as the denominator. However, they were all Maharashtrians and the size of the Marathi group is constant across all observations.

identity with Maharashtrians, the dichotomous indicator of the superordinate identity is coded 1 for them and 0 for Muslims and Parsis. As membership in the common Hindu ingroup and group size are fixed for each ethnic group, I chose four outgroups, for which these two variables are cross-cutting. Gujaratis are a large Hindu minority, Muslims a large non-Hindu minority, Biharis a small Hindu minority, and Parsis a small non-Hindu minority. Another relevant superordinate identity, the Indian national identity, is held constant for all the included ethnic groups.

In addition to the group-level variables described above, the regression model also includes a number of individual-level correlates of discrimination. Social identity theory predicts more ingroup bias and, according to some interpretations, also more outgroup derogation among people with a salient ingroup identity (Brown 2000). The salience of the Marathi identity is measured by how attached to Maharashtra the respondent felt. Using Eurobarometer data, Sinnott (2006) found that this question had more predictive power than other indicators of social identity.

The control variables also include age because older people appeared to be more prejudiced in some studies conducted in North America and Europe (Maykovich 1975; Quillian 1995). Age is also a positive predictor of social distance in the globally collected World Values Survey data (Guiso et al. 2003). In Mumbai, older cohorts may have been affected more directly by a personal experience with the deadly ethnic riots in the 1990s.

Education may correlate with lower prejudice, as it does in many other countries (Quillian 1995; Guiso et al. 2003; Wagner and Zick 2006). I operationalized education as the number of years of schooling. To facilitate interpretation of the regression coefficients, age and the number of years of schooling are centered at their mean values.

Finally, I include dummy variables indicating the respondent's neighborhood and religion.

Due to its length, the questionnaire does not measure other individual attributes that correlate with discriminatory attitudes in surveys – perhaps the most notable example is social dominance orientation (Sidanius 1993). Although these variables could help explain the respondent's general propensity to discriminate, the research question examined in this work does not ask which person discriminates against outgroups, but rather which outgroups the person's discrimination targets. Saying that, one model specification (3) includes individual-level fixed effects to control for unmeasured individual-level confounders. The model (2) does the same for group-level confounders. Finally, the most conservative specification (4) controls for both individual and group-level fixed effects.

4.3 Results

Table 7 shows that the regression results are remarkably stable across various model specifications, including the most stringent model with fixed effects for both group-level and individual-level factors.[20] In all four models, receiving help from outgroup members in the past year lowers social distance to the said outgroup by six to ten percentage points. This effect is statistically significant even after controlling for intergroup contact and other known alternative explanations. As both positive reciprocity

[20] As a robustness check, I replicated this analysis using a proportional odds ordinal logistic regression using the Huber-White method to correct for heteroscedasticity and clustering. This replication addresses potential concerns about the three values of the dependent variable. However, the ordinal logistic regression did not change any substantive conclusions drawn from the generalized linear model presented here.

Table 7: Generalized Linear Model Predicting Discrimination

Model	(1)	(2)	(3)	(4)
Positive	-0.103**	-0.099**	-0.073*	-0.058*
Reciprocity	(0.037)	(0.035)	(0.028)	(0.026)
Intergroup	-0.038*	-0.039*	-0.042**	-0.056***
Contact	(0.018)	(0.016)	(0.013)	(0.011)
Negative	-0.003	-0.014	-0.030.	-0.019
Reciprocity	(0.019)	(0.017)	(0.017)	(0.014)
Economic	-0.010	-0.009	-0.004	-0.006
Threat	(0.013)	(0.013)	(0.009)	(0.008)
Safety Threat	0.000	0.002	0.005	-0.006
	(0.014)	(0.014)	(0.010)	(0.009)
Relative	0.269*		0.232*	
Group Size	(0.120)		(0.092)	
Relative	0.000		0.006	
Group Status	(0.008)		(0.006)	
Shared Hindu	-0.026		-0.029.	
Identity	(0.018)		(0.016)	
Identity	-0.040*	-0.037*		
Salience	(0.020)	(0.019)		
Education	-0.006	-0.011		
	(0.009)	(0.008)		
Age	-0.000	-0.001		
	(0.002)	(0.002)		
Hindu	0.146*	0.170**		
	(0.063)	(0.061)		
Magathane	-0.133**	-0.064		
	(0.046)	(0.048)		
Shivaji Nagar	0.009	0.070.		
	(0.039)	(0.040)		
Intercept	0.791***	0.698***	0.977***	0.442***
Fixed Effects	No	Group	Individual	Both
N	778	838	778	838
Adjusted R^2	0.09	0.09	0.47	0.52

Cluster-robust standard errors (for the models [1] and [2]) and robust standard errors (for the models [3] and [4]) in parentheses.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

and contact are statistically significant, positive reciprocity adds more explanatory power to that of intergroup contact. At the same time, it would not be meaningful to compare the size of the coefficients of reciprocity and contact directly. As intergroup contact is plausibly a causal antecedent of helping behavior, some of its beneficial effect may be channeled through the causal channel of positive reciprocity. This part would not be captured by the reported coefficient of intergroup contact.

Since the survey data are purely observational, the presented analysis cannot identify a causal relationships. It is entirely possible that respondents received help from outgroup members because they did not discriminate against them at the first place. Alternatively, some unmeasured confounder may have influenced both social distance and received generosity. However, the models (2)-(4) control for unmeasured individual-level and group-level confounders without changing the main substantive results. Furthermore, the experiments described in Chapter 3 showed unambiguously that experiencing cooperation with outgroup members caused lower discriminatory attitudes towards the same outgroup as a whole. If the same mechanism operates outside the laboratory, it would create the same pattern as the one observed in the survey data.

Interestingly, being taken advantage by an outgroup member in the past is not associated with higher discrimination of the said outgroup. This finding is consistent with the experimental results presented in the previous chapter, according to which discriminatory attitudes in the treatment groups paired with Muslim defectors were not much different from those in the control group, while attitudes in the groups paired with Muslim cooperators were significantly better. One plausible explanation is that negative reciprocity is too risky for someone who lives in a slum. This argument builds on Molm

(1997), who found that disadvantaged groups rarely use coercion to improve their position because they find this strategy too risky. People in slums are highly dependent on their social networks and have little insurance against a total breakdown of cooperation. Risk aversion may make them reluctant to punish occasional transgressions.

Another plausible explanation is that slum-dwellers are simply used to being taken advantage by others. In the slums, bargaining is an important part of everyday life. People of all castes negotiate prices of vegetables and rice on the market. A large number of transactions, including bribes to government officials, are facilitated by brokers for an often-resented commission. A vast majority of slum-dwellers do not receive a regular salary. Instead, they find temporary jobs in the informal sector with little security of contract enforcement. Wages can be negotiated and renegotiated on a daily basis. In an environment characterized by constant bargaining, it is easy to feel being taken advantage by the other side. Therefore, a positive correlation between this variable and intergroup contact ($r=0.31$) should not come as a big surprise. At the same time, negative contact is not specific to intergroup relations. When asked about being taken advantage by another Maharashtrian, 30 percent of the sample answered that it happens very often.

Only replication in other countries can find out whether the asymmetry between positive and negative reciprocity appears elsewhere in the world or it is an artifact of life in Indian slums. In contrast to the observed non-effect of daily slights, negative reciprocity may become important in extreme cases of defection and therefore should not be completely dismissed as a valid cause of discriminatory behavior. The following words of a victim of ethnic riots in Mumbai illustrates how defection of Muslim neighbors in the times of need contributed to the victim’s subsequent hatred of Muslims

(Punwani 2005, 251):

> "We 14 Hindu families there had a pact with our Muslim neighbors: if our people came to attack, we'd raise an alarm; if their guys came, they would. But they didn't keep the agreement: that's why I hate them. Had they simply driven us out, I wouldn't hate them so much; but they spat at the very plate from which we ate."

Table 7 adds a few more surprises. Unmeasured individual attributes explain almost a half of the variation in discriminatory attitudes. The sampled Marathi-speakers did not differentiate between minorities as much as I expected. Many of them simply preferred the ingroup over all outgroups. Only a small fraction of variance is explained by characteristics of outgroups themselves. Among them, relative group size is the strongest factor. All other things being equal, the number of Maharashtrians willing to discriminate against two largest minorities (Muslims and Gujaratis) is higher by 4 to 5 percentage points than the number of Maharashtrians discriminating against the smaller minorities of Biharis and Parsis. This finding gives empirical support to Posner's prediction that divisions between ethnic groups big enough to seize political power are more salient. According to the survey data from Mumbai, size of the ethnic minority matters in India as much as it does in Sub-Saharan Africa (Posner 2004, 2005) and Western Europe (Quillian 1995).

The replication of Posner's findings outside Sub-Saharan Africa is important enough to warrant a little detour. Blalock (1967) and Quillian (1995) argued that a bigger relative size makes a minority more threatening in the eyes of the dominant group. The survey data from Indian slums support this view. Table A.12 in the Appendix reports the results of two ordinal logistic regression models: one predicting the perceived economic

threat; another predicting the perceived safety threat. Relative group size is the strongest factor in both models. To illustrate the size of the effect, one can estimate the predicted probabilities of different levels of the perceived threat. Increasing the group size from that of Biharis (1.5%) to that of Muslims or Gujaratis (19%) while holding all other variables at their median levels increases the probability of the respondent agreeing or strongly agreeing with the statement that the said outgroup takes jobs away from people like him from 20 to 42 percent. Another highly significant predictor of both threats is negative reciprocity. Respondents reportedly felt threatened by the groups that (they believed) have taken advantage of them. Other groups eliciting perceptions of threat were non-Hindu minorities, minorities enjoying a high social status, and, surprisingly, minorities that the respondent was in frequent contact with. Although competition over jobs and crime are in principle two very different concerns, their statistical predictors look very similar.

Relative group size correlates with both the respondent's hostile intentions and his perception of collective threat. However, there is no evidence that the perceived threat mediates the effect of group size on discrimination. Self-assessed threat is not a statistically significant predictor of discriminatory attitudes in any of the regression models in Table 7. This is true for both economic and safety threats. Neither of them became significant even after the objective measure of the relative group size and the other subjective measure of the perceived threat were dropped from the regression equation (regression results not reported here). This surprising results contradicts an earlier study conducted among students in the Indian state of Orissa by Tausch et al. (2009). In their analysis, realistic threat was associated with social distance, though much

less among Hindus than among Muslims. Unfortunately, their composite indicator does not differentiate between economic and political threats.

At first glance, insignificance of the perceived threat to access to jobs is surprising in a city, in which two major political parties routinely accuse minorities of stealing jobs from the Marathi-speaking "sons of the soil." Ethnic conflict over jobs has a long and troubled history in Mumbai. Even before Shiv Sena launched its campaign of intimidation to secure jobs for Maharashtrians, some ethnic riots originated as ordinary industrial strikes and escalated into ethnic violence after employers tried to recruit blacklegs from a caste or religion different from that of the striking employees (Noronha 2005). However, Marathi slum-dwellers in contemporary Mumbai see through the political parties' rhetoric and know that their principal competitors on the job market are not Muslims or Gujaratis, but rather other Marathi-speakers. According to my survey data, 62 percent of the respondents strongly agreed with the statement: "Maharashtrians take jobs away from people like me." Only 10 percent strongly agreed with the similar statement about Muslims and 8 percent about Gujaratis. Ethnic conflict in the slums of Mumbai is not driven by competition for scarce jobs because this competition actually often pits members of the same ethnic group against each other.

Insignificance of the perceived threat to safety is even more counter-intuitive. In a city that is generally considered India's capital of organized crime, sensationalist media pay a lot of attention to the crimes committed by Muslims and almost automatically – and without any proof – link them to Islamist terrorist networks (Shaban 2010). Despite this dominant narrative, more respondents blamed Maharashtrians (76%) rather than Muslims (60%) for the increased crime rate. In a way, Indians surveyed for this research seem to

be surprisingly similar to the Dutch subjects surveyed by Sniderman et al. (2004), who found that threats to safety were actually the least important predictor of hostility towards immigrants in the Netherlands.

Table 7 provides mixed evidence in support of the common ingroup identity model. Respondents seemed to discriminate less against the groups sharing the common Hindu identity with them, but the corresponding coefficients were not statistically significant at the customary 95% level. A replication using a subsample restricted to self-reported Hindus did not make the superordinate Hindu identity any more significant. On the other hand, Table 7 shows that in comparison to Buddhists, Hindus tend to discriminate more across the board.

A salient Marathi identity correlates with discrimination against non-Marathi minorities, except that the correlation is in the opposite direction to that predicted by social identity theory. After controlling for other factors, salience of the ingroup identity is negatively associated with discriminatory attitudes in general. The effect is stronger for discrimination against Gujaratis and Parsis. As these two groups are considered part of Mumbai's traditional bourgeoisie (Hansen 2001), while Biharis are recent immigrants and Muslims represent the Other in the Marathi historiography, this result may indicate presence of a narrower superordinate identity, based on belonging to the city of Mumbai or to the state of Maharashtra, that influences people with a strong attachment to Maharashtra.

Respondents did not discriminate between groups based on the perceived group status. Discrimination against the minorities with the lowest social status was no different from discrimination against other groups. Unfortunately, as social stratification in India is

quite rigid, the data gathered in the slums of Mumbai cannot test the hypothesized effect of status reversals.

The regression coefficient for age is close to zero. This result contradicts some of the previous studies that found older people more prejudiced (Guiso et al. 2003). Although almost three quarters of the sample are old enough to remember the bloody ethnic riots of the 1990s, this negative experience does not appear to influence discriminatory attitudes in any systematic way 18 years later.

Education is another control variable without the effect predicted by the literature. According to Wagner and Zick (2006), a negative correlation between education and prejudice is a robust empirical observation in Western Europe. However, my analysis questions whether we can expect the same beneficial effect in India. The null effect of education can be probably attributed to the differences between the average schools in Europe and in India. Banerjee and Duflo (2011) described the abysmal state of Indian schools, in which teachers miss on average more than one out of five days of work and when they come to school, “they are often found drinking tea, reading the newspaper, or talking to a colleague.” As a result, 35 percent of Indian children in the seven-to-fourteen age group surveyed for the Annual State of Education Report (ASER) could not read a simple paragraph (Banerjee and Duflo 2011). Although the average respondent in my Mumbai survey completed 11 years of schooling, differences between schools in India and Europe make a direct application of Western theories about education reducing prejudice somewhat problematic.

4.4 Conclusion

The analysis of survey data presented in this chapter showed that the causal relationships identified by the laboratory experiments in the previous chapter map onto real-life patterns of ethnic discrimination. Positive reciprocity is associated with lower discrimination against four different ethnic minorities living in Mumbai. The correlation remains significant after controlling for intergroup contact and other alternative explanations. At the same time, the survey data offers no evidence of a similar effect by negative reciprocity.

Reciprocity adds more explanatory power to models of selective discrimination based on relative group size. The argument advanced by this dissertation can help explain discrimination of ethnic groups that are too small, weak, or politically disenfranchised to be considered serious contenders by the dominant group. Examples of such groups include not only Muslims and Christians in many parts of India, but also Roma and foreign immigrants in Europe.

Positive reciprocity is an important factor despite a lot of competition over scarce resources in economically and socially disadvantaged slum neighborhoods of Mumbai. It is a stronger factor than threat perception, identity salience, or education. If this finding can be replicated elsewhere, encouraging cooperation across group boundaries may become a means to transcend ethnic conflicts based on real material interests.

Chapter 5. Ethnic Diversity and Public Goods Provision

The previous chapters tested observable implications of the theory on the data collected in Indian slums. Using cross-national survey data, this chapter extends the analysis beyond India. Examining whether generalized reciprocity moderates the effect of ethnic diversity on people's willingness to contribute to public goods around the world, it also applies the proposed theory on one of the greatest puzzles in the economic development literature.

5.1 Public Goods Provision in Ethnically Heterogeneous Countries

A plethora of studies from all around the world indicates that ethnic diversity sometimes – but not always – leads to suboptimal public goods provision and hinders economic development. Since Easterly and Levine (1997) blamed ethnic diversity for low economic growth in Sub-Saharan Africa and La Porta et al. (1999) for bad governance, economists have detected a negative correlation between ethnic heterogeneity and public goods provision both cross-nationally (Alesina et al. 2001) and within individual countries as different as the United States (Alesina et al. 1999), India (Banerjee et al. 2005), and Kenya (Miguel and Gugerty 2005). At the micro-level, Okten and Osili (2004) found that households in ethnically mixed communities are less likely to contribute to voluntary organizations that generate public goods in Indonesia. Applying a multilevel model on cross-national survey data, Lago-Peñas and Lago-Peñas (2010) showed that ethno-linguistic fractionalization is also associated with a lower tax morale.

Despite the seeming robustness of the negative association between ethnic diversity and public goods provision claimed by some of the earliest studies (La Porta et al. 1999), many cases deviate from this pattern. Ethnic heterogeneity leads to different outcomes even in some very similar cases, such as comparable districts in Kenya and Tanzania (Miguel 2004). The results also depend on the type of social heterogeneity, with racial diversity possibly having more negative effects than cultural diversity (Alesina and La Ferrara 2005). As Alesina and La Ferrara (2005, 794) concluded in their comprehensive survey of the relevant literature:

> "Rich democratic societies work well with diversity, in the case of the United States very well in terms of growth and productivity. Even within the developing world, similar levels of ethnic diversity are associated with very different degrees of conflict and interethnic cooperation."

Existing theories do not explain very well under what conditions ethnic diversity prevents multiethnic societies from generating enough public goods for their members. The causal mechanism proposed by early studies – ethnically fragmented societies not able to agree on which public goods to produce due to divergent preferences – did not include any intervening variables or boundary conditions (Easterly and Levine 1997; Alesina et al. 1999; Kinder and Winter 2001). Habyarimana et al. (2009) tested this and several other causal mechanisms explaining why ethnically homogenous communities succeed more often in providing their members with public goods. However, their book addressed the question *how* ethnic diversity undermines public goods provision and not *under what conditions* it does not. This chapter offers an answer to the latter.

Few studies have suggested what might moderate the negative effect of ethnic

heterogeneity. Comparing Kenya and Tanzania, Miguel (2004) explained why ethnic diversity has no impact on local public goods provision in Tanzania by the inclusive national identity. He argued that Tanzanian political leaders managed to bridge ethnic divisions in their country chiefly by promoting Swahili as the common language. Similarly, Glennerster et al. (2010) highlighted the role of a common lingua franca in Liberia, where, as they found, ethnic heterogeneity did not influence local public goods provision.

Collier (2000) argued that ethnic fractionalization has negative effects on economic growth and productivity only in nondemocratic regimes, while democracies manage to cope better with ethnic diversity. Alesina and La Ferrara (2005) provided more empirical support for this argument. At the same time, they also pointed out that ethnic heterogeneity and lower growth are associated more strongly in poorer countries as well as in less affluent counties of the United States. As economic development is highly correlated with democracy, it is not easy to disentangle the moderating effect of wealth from that of political regime.

This dissertation highlights a different factor likely to moderate the effect of ethnic diversity on public goods provision. Chapter 1 argued that people are able to cooperate across ethnic lines if they trust their partners to reciprocate cooperative behavior. Mirroring the results of the laboratory public goods experiment described in Chapter 3, expectations of reciprocity should lead to greater public goods provision also in real life. And they do. For example, Frey and Torgler (2007) observed a strong correlation between perceived tax evasion and tax morale of European survey respondents. This chapter does not look at the effect of conditional cooperation, but rather

at the interaction between conditional cooperation and ethnic diversity. Do beliefs about others' cooperativeness motivate people to invest in public goods despite ethnic heterogeneity? Unlike alternative mechanisms discussed in previous studies, the argument proposed by this paper sees the relationship between ethnic heterogeneity and public goods provision as conditional on generalized reciprocity.

Table 1 in Chapter 1 established generalized reciprocity as one of possible stable states of the society. In the societies with a strong social norm of generalized reciprocity, rational actors have an incentive to cooperate regardless of the group membership of their partners because they expect ingroup and outgroup members to cooperate with the same (high) probability. Therefore, ethnic divisions should not prevent such a society from generating enough public goods for its members.

At the micro-level, I expect trustful people to be equally willing to contribute to public goods regardless of how ethnically fragmented their society is. If they do not trust strangers to be cooperative, however, I expect them to contribute less as ethnic heterogeneity increases. In other words, we should observe a significant interaction between ethnic heterogeneity and generalized trust. The paper tests this hypothesis on worldwide survey data from 87 countries and dependencies.

Chapter 3 used a survey question about trust of a particular ethnic group to measure expectations of reciprocity regarding the said group. As I explain in the next section, this chapter uses a survey question about generalized trust to measure expectations of generalized reciprocity. Unlike trust particular to a specific group, "generalized trust reflects a bond that people share across a society and across economic and ethnic groups, religions, and races" (Rothstein and Uslaner 2005, 45). It can be

defined as horizontal trust among people and it encompasses strangers and unknown groups as well. Freitag and Bühlmann (2009, 1540) considered generalized trust to be an indicator of the "environment of general reciprocity" that "makes cooperation possible, and minimizes the risks involved in the act of trust."

Generalized trust has an important advantage that several big national and cross-national surveys attempt to measure it in a systematic way. However, the standard proxy for generalized trust only roughly approximates expectations of generalized reciprocity, as Chapter 1 defined them. The dyadic example summarized in Table 1 reserved the term generalized reciprocity for the case, in which a person trusts both ingroup and outgroup members to reciprocate cooperative behavior. Applying the same logic to multiple groups, expectations of reciprocity are generalized if all relevant groups can be expected to cooperate. On the other hand, surveys usually ask the respondent whether "most people" can be trusted. The conventional wording does not help distinguish a situation, in which a member of the ethnic majority trusts other ingroup members – and thus "most people" – and yet refuses to contribute to public goods provision because she does not want to throw resources on a mistrusted minority. Broadly speaking, the standard survey measure covers cases, in which the respondent trusts members of the groups that comprise a majority of the population without necessarily extending her trust to the society as a whole. To illustrate this discrepancy with empirical data, 45 percent of the respondents in the Mumbai survey, who declared that they trusted most people, also said they did not trust Muslims.

Another problem with the generalized trust question is that it does not differentiate between different levels, at which contributions to public goods happen. A

citizen paying local property taxes may be more concerned specifically with trust in other residents of her school district rather than with trust in humanity as a whole or in most people she knows. Unfortunately, there is no comprehensive cross-national source of information about group-specific and contextualized trust. As I could not find a better proxy, I had to use the imperfect survey measure of generalized trust.

5.2 Research design

The main goal of this chapter is to model individual willingness to contribute to public goods provision as a function of ethnic diversity and generalized trust. The unit of analysis is the individual respondent and the data come from the World Values Survey. The World Value Survey (WVS) is a large-scale cross-national survey organized in six waves: 1981-1984, 1989-1993, 1994-1999, 1999-2004, 2005-2008, and 2008-2010. As the results of the last wave have not been made public yet, this study pools the data from all surveys between 1981 and 2008. Table A.13 in the appendix shows which countries are included in what years. Table A.14 describes the distribution of the variables.

The main analysis uses the weighted generalized linear model, with the weights provided by the national WVS teams to make the samples more representative of the population of each country. The multilevel model at the end of the chapter is weighted in the same way. As I was interested in variation across countries, I did not use weights proportional to the total population of the country in the pooled data. Weighting by population of the country would practically discard variation in ethnic fractionalization among small countries and the overall results would be driven by few biggest countries,

such as China and the United States.

Public Goods. The dependent variable of interest is the respondent's willingness to invest in public goods. The WVS does not ask directly whether the respondent is a free rider or if she contributes to public goods provision. A direct question would probably elicit a large number of socially desirable – though untrue – responses. Due to the prevailing social norms, respondents would be unlikely to disclose free riding behavior. Instead of a more direct question producing biased answers, the WVS asks about people's acceptance of free-riding behavior in general. The assumption is that people approving of free riding are also more likely to act in this way. Conversely, people rejecting free riding are plausibly more likely to contribute to public goods produced by their society.

Two relevant WVS questions ask about contributions to public goods in the form of paying taxes and paying a fare on public transportation. The first item says: "Please tell me for each of the following statements whether you think it can always be justified, never be justified, or something in between, using this card. Cheating on taxes if you have a chance." The second item asks the same question about "avoiding a fare on public transport." Both indicators are positively correlated (r=0.51) in the WVS data. Therefore, this chapter's dependent variable is the arithmetic average of the two elements described above with the scale inverse of that of the original WVS variables: 10 denotes the highest and 1 the lowest willingness to contribute to public goods.

Uslaner (1999) used these two WVS questions as part of his indicator of "moral behavior." Knack and Keefer (1997) included them in their "index of civic cooperation." Taking a leap from observed attitudes to what motivates them, Letki (2006, 306) argued that tax compliance and paying one's due on public transportation are proxies for "civic

morality" that "leads citizens to maximize public rather than private gains, therefore deterring them from engaging in corruption and free-riding." These three cited studies merged the two questions about paying one's taxes and a fare on public transportation with several other WVS questions to form composite indices of "moral behavior," "civic cooperation," or "civic morality." My study focuses only on the public goods aspect.

Trust. Similarly to other large-scale surveys, the WVS measures generalized trust by the question: "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?" Answers are coded 1 ("Most people can be trusted") or 0 ("Can´t be too careful").

Although Glaeser et al. (2000) found that the question was associated with greater trustworthiness – not trustfulness – among the subjects playing a laboratory trust game, Cox et al. (2009) observed a positive correlation between trust measured by the survey question and trustful behavior in lab. Knack and Keefer (1997) found that average generalized trust measured by the survey question was associated with return rates in wallet-drop experiments in the same territory. As I pointed out above, it is not completely clear how respondents understand the term "most people" despite these correlations with behavior.

Ethnic Fractionalization. Ethnic diversity is measured at the country level. Although there are several competing measures of ethnic heterogeneity, most of them are based on the Herfindahl concentration formula:

$$ELF_j = 1 - \sum_{i=1}^{N} s_{ij}^2$$

where ELF_j is ethnic fractionalization of country j, s_{ij} is the share of group i in country j,

and *N* is the number of groups.[21] This formula essentially measures the probability that two randomly selected individuals from the population will be from different groups.

Easterly and Levine (1997) used the index of ethnolinguistic fractionalization (ELF), based on the data from *Atlas Narodov Mira*, a Soviet ethnographic atlas published in 1964. The Soviet source was originally popularized by Taylor et al. (1972). Since then, ELF has become a standard measure of ethnic heterogeneity in quantitative cross-national studies. Fearon (2003) built an alternative – and more up-to-date – ethnic fractionalization score from a variety of different secondary sources. Finally, Alesina et al. (2003, 159) created an ethnic fractionalization index combining "racial and linguistic characteristics" in order to identify the most meaningful ethnic categories in each country. The primary source was *Encyclopedia Britannica*, complemented with the *CIA World Factbook*, national census data, and other sources. Due to an overlap of their sources, Alesina et al. (2003) and Fearon (2003) produced very similar indices. Alesina's index is probably the most widely-used measure of ethnic fractionalization in the fields of economics and political economy. Because of its comprehensive coverage of countries and wide use in the literature, I decided to adopt Alesina's ethnic fractionalization index as a proxy for ethnic diversity.

Just as virtually all ethnic fractionalization indices, the variable is computed at the

[21] Notable exceptions include indices measuring ethnic polarization instead of ethnic fractionalization. For example, Montalvo and Reynal-Querol (2005) developed an index of ethnic polarization and Cederman and Girardin (2007) proposed an alternative N* index of ethnonationalist exclusiveness. Montalvo and Reynal-Querol (2005) argued that polarization is a better predictor of ethnic conflict than fractionalization because its non-monotonicity approaches the well-known inverted-U relationship between ethnic heterogeneity and the probability of ethnic conflict. However, this dissertation chapter focuses on a different question – whether an individual is willing to cooperate with a large group of strangers in public goods provision. Fractionalization is arguably part of a rational calculus whether to cooperate. If I trust ingroup members more than outgroup members and I am deciding whether to pay taxes, the share of ingroup in the total population matters. If everyone faces the same decision, the probability of cooperating should decrease with the probability that two randomly selected individuals from the population will be from different groups, that is ethnic fractionalization.

country level and does not vary in time. Although Laitin and Posner (2001) criticized ethnic fractionalization indices as disregarding the fact that ethnic identities can change over time, the lack of temporal variation should not be a big problem for the short time window 1981-2008 explored in this study.

The regression equations presented in this chapter also control for a number of variables possibly confounding the effect of trust on the willingness to contribute to public goods provision. They are listed below.

Acceptance of Bribe. Trustful people may refrain from free raiding not because they expect strangers to reciprocate cooperation, but because of some innate personal attribute, such as altruism or natural law abidance. In fact, Guiso et al. (2003) used components of my dependent variable (justification of cheating on taxes and avoiding a fare on public transportation) as proxies for people's attitudes to legal norms. To control for this confounding effect, I included another variable on the right side of the regression equation: acceptance of bribe.[22] Controlling for this variable should disentangle the public goods element of the questions used to construct the dependent variable from law compliance measured by the corruption question.

Confidence in Government. Apart from horizontal generalized trust in fellow citizens, there also exists vertical trust between citizens and the state. Scholz and Lubell (1998) argued that vertical (political) trust creates focal points for cooperative solutions and horizontal (social) trust reduces the costs of enforcement of collective solutions. They also found that political trust, in the form of confidence in government institutions, is

[22] The exact wording of the WVS question is: "Please tell me for each of the following statements whether you think it can always be justified, never be justified, or something in between, using this card. (Read out statements. Code one answer for each statement). Someone accepting a bribe in the course of their duties."

empirically associated with higher tax compliance. This finding was successfully replicated by Letki (2006), Marien and Hooghe (2010), and other studies. The correlation between political and social trust is typically weak because confidence in government institutions varies to a great degree with partisanship: people who support the ideology of the ruling party are also more likely to express confidence in the government (Rothstein and Stolle 2008). However, the regression models presented in this chapter control for confidence in political institutions, as this variable may confound the effect of trust on tax compliance.

Similarly to Marien and Hooghe (2010), I constructed a composite variable of confidence in political institution from a battery of questions included in the WVS data. The survey question asks: "I am going to name a number of organisations. For each one, could you tell me how much confidence you have in them: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all?" The items included in the composite variable are the armed forces, the police, parliament, the civil service, the government, and justice system. Missing values were imputed based on answers to non-missing questions from this battery. The resulting composite variable is an arithmetic average of all the answers rescaled so the higher numbers indicate more confidence in government institutions.

Church Attendance. Listhaug and Miller (1985) and Guiso et al. (2003) found religious people to be less likely to approve of cheating on taxes. Based on a comprehensive review of more recent studies, Lago-Peñas and Lago-Peñas (2010) consider this result to be one of the most robust findings in the tax compliance literature. At the same time, religion also influences generalized trust (Guiso et al. 2006), creating a

possible confounding problem. When Torgler (2006) concluded that religiosity raises tax morale, he analyzed a variety of WVS questions. Unfortunately, most of them were asked only in some of the surveys included in my analysis. One of Torgler's indicators of religiosity, church attendance, has an advantage of appearing frequently in the WVS. As it produces the least number of missing values, I chose it for this study. The question asked: "Apart from weddings, funerals and christenings, about how often do you attend religious services these days?" The scale runs from 0 ("never, practically never") to 7 ("more than once a week").

I also added a host of individual-level demographic variables that influence the dependent variable and may correlate with trust: *Sex*, *Age*, *Marital Status*, *Education*, and *Income*.[23] A great number of studies have found that tax compliance tends to be higher among older people, women, and married people (Uslaner 1999; Guiso et al. 2003; Torgler 2006). Age, gender, and marital status seem to be the most consistent demographic predictors of tax morale in the literature (Lago-Peñas and Lago-Peñas 2010). Listhaug and Miller (1985) and Guiso et al. (2003) also found people with higher income to be more likely to cheat on taxes. The effect of education is much less consistent (Uslaner 1999; Torgler 2006; Lago-Peñas and Lago-Peñas 2010).

Tax Revenue. As Rose (1984, 122) put it succinctly: "Within any given country, the level of tax resistance is likely to be greater when taxes are high rather than low." Simultaneously, tax rates seem to correlate with generalized trust due to relatively heavy taxation in the exceptionally trustful Nordic countries. The control variables thus include

[23] Sex is coded 1 for male and 0 for female respondents. Age is coded in the number of years. Marital status differentiates between married (1) and unmarried (0) people. The highest educational level attained has eight categories, as provided by the WVS. The scale of income uses ten categories specific for each country. Therefore, this variable measures within-country, but not between-country variation.

the overall tax revenue as a share of the country's GDP (in percent). The variable is measured at the country-level. Given the high measurement error and the fact that the World Bank does not report this variable for all the years, in which the WVS was run, I computed country averages for each survey wave. So, for example, the missing tax revenue of Georgia in 1996 was imputed with Georgia's average tax revenue during the whole survey wave (1994-1999). Unfortunately, no tax revenue data could be found for the first wave of the WVS (1981-1984).

Democracy. Political institutions may be another country-level confounder. La Porta et al. (1999) found that ethnic diversity is associated simultaneously with bad governance, low public goods provision, low tax compliance, and less political freedom. Rothstein and Stolle (2008, 453) showed that "countries with high levels of generalized trust also have the most effective and impartial institutions and the longest experiences with democracy." Tabellini (2010) sought an explanation in history: regions of Europe with less legal constraints on the executive in the past tend to be characterized by lower generalized trust in the present. As my variables of interest (ethnic diversity, generalized trust, public goods) are all correlated with political institutions, I included the Polity IV score of the country at the time of the survey as a control variable. The scale runs from -10 (autocracy) to 10 (full democracy).

Some of the effect of ethnic diversity and trust is probably channeled through the control variables. For example, distrustful people usually have less confidence in political institutions as well. At the same time, they are less willing to pay their taxes. If this is the case, the reported coefficient of generalized trust may underestimate its effect because some of it will appear as part of the coefficient of confidence in government. That is why

I report coefficients of the main explanatory variables with and without covariates.

5.3 Results

Quantitative analysis of the cross-national survey data identified the hypothesized interaction between ethnic diversity and generalized trust. Figure 6 summarizes concisely the main finding. It shows the predicted values of the willingness to contribute to public goods as a function of ethnic fractionalization and generalized trust, while holding all other variables constant at their median values. The solid line represents trustful people; the dotted line those who do not trust strangers. Shaded areas around each line show 95% confidence intervals. The rug plot along the x-axis describes the distribution of countries based on their ethnic fractionalization index.

The down-sloping dotted line indicates that people with no expectations of generalized reciprocity are less willing to contribute to public goods as ethnic heterogeneity of the country increases. This result mirrors earlier findings by Alesina et al. (2001), Lago-Peñas and Lago-Peñas (2010), and other studies. However, the novel finding is that ethnic diversity interacts with generalized trust, as predicted by the theory outlined in this dissertation. The negative effect of ethnic fractionalization on public goods provision almost disappears among trustful people. The predicted values (represented by the solid line) approach the ideal flat line corresponding to the null effect of ethnic fractionalization. The difference between the two lines is more significant for highly diverse countries, in which the effect of ethnic heterogeneity on public goods would be otherwise the most detrimental.

Figure 6: Predicted Willingness to Contribute to Public Goods

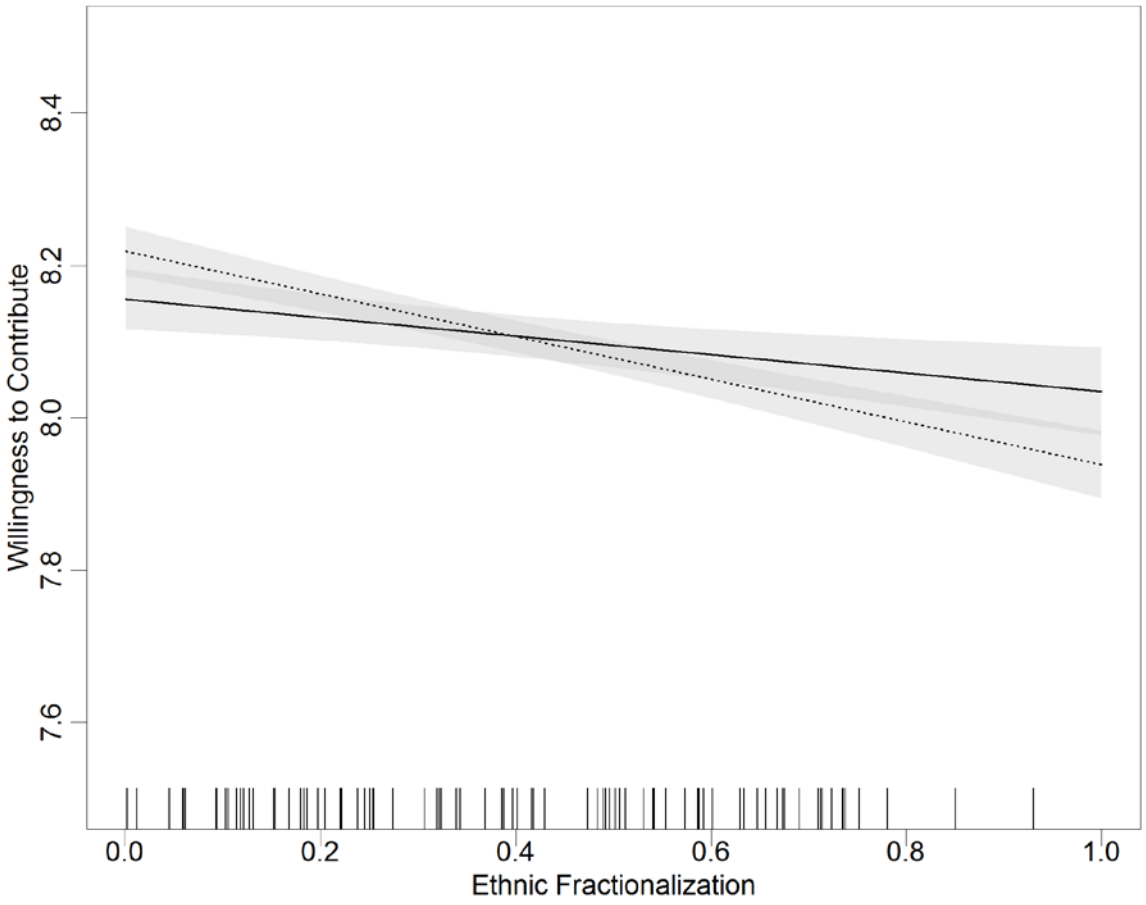


Figure 6 is based on the full regression model with all the control variables (model 3 in Table 8). But let us take a step back and start with the simplest model (1) in the same table. As found in many previous studies, people in ethnically diverse countries are less likely to contribute to public goods. However, the model (1) shows that the relationship between these two variables is moderated by generalized trust. People with expectations of generalized reciprocity are more willing to contribute to public goods than their distrustful compatriots as ethnic fractionalization increases. This basic finding remains robust after adding more control variables in models (2)-(4).

Table 8: Weighted GLM Predicting Willingness to Contribute to Public Goods

Model	(1)	(2)	(3)	(4)
Ethnic Frac.	-0.441***	-0.113***	-0.280***	-0.235***
	(0.026)	(0.024)	(0.033)	(0.028)
Trust	0.084***	-0.021	-0.063**	0.000
	(0.018)	(0.018)	(0.021)	(0.012)
E. F. * Trust	0.111*	0.087*	0.159***	
	(0.043)	(0.041)	(0.047)	
Acceptance of		-0.545***	-0.542***	-0.542***
Bribe		(0.004)	(0.005)	(0.005)
Male		-0.040***	-0.038***	-0.038***
		(0.009)	(0.011)	(0.011)
Age		0.005***	0.006***	0.006***
		(0.000)	(0.000)	(0.000)
Education		-0.012***	-0.005*	-0.005*
		(0.002)	(0.003)	(0.003)
Income		-0.001	-0.007**	-0.007**
		(0.002)	(0.003)	(0.003)
Married		0.136***	0.095***	0.095***
		(0.010)	(0.012)	(0.012)
Church		0.059***	0.060***	0.060***
Attendance		(0.002)	(0.002)	(0.002)
Confidence in		0.293***	0.257***	0.257***
Government		(0.008)	(0.010)	(0.010)
Tax Revenue			-0.015***	-0.016***
			(0.001)	(0.001)
Democracy			-0.005***	-0.005***
			(0.001)	(0.001)
Intercept	7.738***	7.446***	7.856***	7.840***
N	206809	155142	109345	109345

Cluster-robust standard errors in parentheses.
. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

The model (2) includes individual-level covariates. Since not every question was asked in every survey, the number of respondents drops by 50,000. Despite all these changes, the interaction term between ethnic fractionalization and generalized trust is still statistically significant and in the predicted direction. In other words, the main finding of

the study is robust in light of individual-level confounders. Similarly, the main effect of ethnic diversity remains negative and statistically significant.

The interaction term remains statistically significant and positive even after controlling for country-level confounders of tax revenue and democracy in the model (3). In this model, N drops again by about a third because the country-level variables are missing for some of the territories included in the WVS. The model (3) increases confidence that the reported effect of ethnic diversity and its interaction with generalized trust are disentangled from the effect of political institutions that they may correlate with.

The model (4) shows what happens if we exclude the interaction term, as previous studies of public goods provision did. The coefficient of ethnic fractionalization is negative and highly significant, but the coefficient of generalized trust is essentially equal to zero. Although trust is a positive predictor of contributions to public goods in the model (1), more comprehensive models with covariates show that almost all of its explanatory power can be attributed to various confounders, such as confidence in political institutions or law abidance. However, even while insignificant on its own after controlling for possible confounders, generalized trust still moderates ethnic heterogeneity in the model (3). A safe interpretation of these results is that although generalized trust does not necessarily increase individual contributions to public goods, it substantially reduces the negative effect of ethnic diversity.

The regression analysis presented in Table 8 mostly confirms findings of previous studies regarding individual-level factors. Older, married, and religious people are more likely to invest in public goods. Women tend to contribute more than men. Richer and better educated people are more reluctant to contribute, which may be explained by their

greater tax burden under progressive taxation or by their knowledge of how to evade taxes. Quite predictably, public goods provision correlates positively with confidence in political institutions and negatively with the overall level of taxation. What is a bit more puzzling, citizens of democratic countries seem to be less willing to contribute on average.

Standard errors in all four models were clustered at the level of individual surveys because error terms of respondents surveyed in the same country and in the same year may be correlated. Coefficients from the weighted GLM are easy to interpret and clustering of standard errors removes the concern that the reported errors are underestimated. However, a more appropriate way to analyze data with individual-level and group-level predictors is through multilevel modeling. A major advantage is that variables from different levels can be analyzed simultaneously.

Therefore, I replicated the analysis using a weighted random-intercept multilevel model of the willingness to contribute to public goods as a function of all the individual- and country-level variables included in the weighted GLM model (3). The used multilevel model allows intercepts to vary across 841 regions within the 87 sampled countries. Different intercepts capture different unmeasured conditions in these regions. For example, Putnam et al. (1993) famously showed that North Italy inherited a more cooperative culture than South Italy. The multilevel model can estimate a different (probably higher, in this case) intercept for North Italy. Due to different historical circumstances, regional differences in public goods provision are substantial (Tabellini 2010; Lago-Peñas and Lago-Peñas 2010). Unlike the GLM model in Table 8, the random-intercept model takes them into account.

Table A.15 in the appendix summarizes the results. On the one hand, the interaction between ethnic fractionalization and generalized trust is in the predicted positive direction, while the main effect of ethnic fractionalization is in the predicted negative direction. On the other hand, in this model specification, the interaction term is no longer statistically significant at the conventional threshold of 5% (p-value=0.076). The multilevel model thus offers only weak evidence for the inferences drawn from the weighted GLM models presented above.

5.4 Conclusion

The negative effect of ethnic diversity on public goods provision has become an accepted wisdom in the economic literature. However, not all ethnically mixed societies fare badly. Consequently, the question which communities can escape the supposed trap of ethnic fragmentation has become of crucial importance. As Rothstein and Stolle (2008, 441) stated: “That citizens in some countries, regions, cities, or villages are able to trust each other and thereby solve many of their collective action problems while others are not turns out to be one of the most interesting puzzles in the social sciences.”

Applying a new theory on an old problem, the chapter offers a solution to this puzzle. The cross-national analysis of survey data showed that whereas the country’s ethnic heterogeneity is associated with citizens’ lower willingness to invest in public goods provision, expectations of generalized reciprocity significantly reduce this negative effect. The dissertation adds a new item to the short list of variables that moderate the relationship between ethnic diversity and public goods provision. Whereas other known

moderators – common language (Miguel 2004), democratic regime (Collier 2000), and economic development (Alesina and La Ferrara 2005) – are macro-scale and difficult to manipulate, generalized trust can be increased more easily at the individual level. This unique feature has profound implications for future experimental research as well as policy-making.

The chapter also provides an important empirical test of the theory explained in Chapter 1. Departing from the dissertation's focus on ethnic discrimination in India, it includes a different dependent variable (contributions to public goods) and uses data from 87 countries all around the world.

Chapter 6. Conclusions

6.1 Summary of Findings

The dissertation provides empirical evidence supporting the observable implications outlined in Chapter 1. The lab-in-the-field experiments in the slums of Mumbai showed that positive indirect reciprocity experienced during interactions with individual Muslims in the laboratory increased trust in and reduced discrimination against the Muslim outgroup as a whole. The experimental intervention was effective even in a subgroup of supporters of local nationalist Hindu parties, infamous for organizing ethnic riots against Muslims. The survey demonstrated a similar positive correlation between positive reciprocity in real life and discriminatory attitudes towards four different ethnic minorities living in Mumbai. The statistical relationship remained significant after controlling for intergroup contact and various alternative explanations, including relative group size, perception of threat, relative social status, salience of ingroup identity, and superordinate identity. Finally, the cross-national section of the dissertation extended the analysis beyond India. Using surveys from 87 countries, it showed that generalized trust moderates the negative effect of ethnic diversity on people's willingness to contribute to public goods.

As most of the crucial evidence comes from field research in Indian slums, I hope that future extensions of this research to other populations will assess to what extent the lessons learned in the slums of Mumbai are generalizable to other contexts. It would be especially interesting to examine the effect of reciprocity among men and women in more

peaceful and economically more developed countries. The dissertation also left unanswered the intriguing question of long-term causal effects of reciprocity.

6.2 Theoretical Contributions

The dissertation advances reciprocity as a determinant of relations between groups. This new understanding of intergroup relations has implications especially for students of ethnicity and economic development. The presented work sheds light on one micro-level mechanism (linking expectations of cooperative behavior and discrimination) in a causal chain leading to macro-level phenomena, such as ethnic violence and public goods provision. More research is needed to follow other causal links from this level of analysis to the macro-scale phenomena that interest most political scientists.

6.2.1 Contributions to the Literature on Ethnicity

The dissertation develops and tests a new explanation of ethnic discrimination. Using both experimental and survey data, it demonstrates that positive reciprocity can improve relations between groups. At the same time, there is no clear evidence in the data that negative reciprocity has necessarily the opposite effect. Increasing expectations of cooperative behavior from outsiders as an intervention is a new addition to the prejudice reduction literature, until now preoccupied mostly with intergroup contact, multicultural education, perspective-taking, and other approaches (Paluck and Green 2009).

Despite much discrimination in everyday life, subjects displayed little

ethnocentrism in the laboratory. When anonymity removed any fear of social sanctions, people did not prefer coethnics in the dictator game or the public goods game. Curiously, there is almost no evidence of ethnocentric altruism in the experimental data gathered in Mumbai. Altruism also remained unaffected by positive and negative experience with another ethnic group. Although both elements of other-regarding preferences (reciprocity and altruism) have received a lot of attention in the literature, this study suggests that we should focus more on reciprocity. The dissertation also contributes to the growing body of evidence that ingroup bias is largely driven by social sanctions and not so much by natural preference for coethnics (Yamagishi and Mifune 2008; Habyarimana et al. 2009).

In contrast to the usual understanding of social identity theory, a salient ingroup identity was not associated with more outgroup discrimination in the analyzed sample. In fact, after controlling for other factors, a salient Marathi identity seemed to be associated with lower discrimination in some model specifications. Neither did identity salience increase donations to other Hindus in the dictator game.

6.2.2 Contributions to the Literature on Economic Development

The dissertation provides evidence from three different sources that expectations of reciprocity facilitate collective action in ethnically diverse societies. The laboratory experiment run in the slums showed that cooperation across ethnic lines was possible even among the poor living in the conflict-ridden city of Mumbai. After I manipulated expectations of reciprocity, ethnically heterogeneous groups produced as much public goods as the homogeneous ones in the public goods game. The survey conducted in the

same environment demonstrated that helping behavior in the slums crosses ethnic lines in real life as well. Positive reciprocity \ leads to greater trust, which in turn enables more future cooperation. As previous works have discovered substantial positive effects of trust on economic and social outcomes, it was important to demonstrate experimentally that interpersonal trust can be increased by experiencing others' cooperative behavior. The analysis of the World Values Survey data for this dissertation also showed that generalized trust moderates the negative relationship between ethnic diversity and the willingness to invest in public goods. All these findings suggest that ethnic diversity is not a poverty trap, but its negative effects on economic development can be reduced by micro-level interventions.

6.3 Methodological Contributions

The dissertation also makes several methodological contributions to experimental research in social sciences. Probably the most important contribution is that the laboratory experiment in Mumbai used a behavioral game as treatment and measured the dependent variable by a post-treatment survey. This approach opens up a new venue for experimentalists interested in how subjects' experience during laboratory games changes their attitudes and behavior after the experiment is over. The strong findings also raise worries that any uncontrolled behavior by other players in repeated games may have a strong effect on the subject. Green and Tusicisny (2012) explain in more detail what changes to research design and analysis of repeated games are needed to produce unbiased estimates of the ATE if this is the case.

The dissertation helps remedy the lack of experimental studies of prejudice reduction in post-conflict zones and in non-western settings that Paluck and Green (2009) identified in their exhaustive survey of literature. Following a recent trend among lab-in-the-field experimentalists, the dissertation also uses a sample representative of a politically relevant population instead of a convenience sample consisting of undergraduate students.

Questionable external validity is traditionally one of the main criticisms of experimental research in political science. The dissertation increased the external validity of its findings not only by using a random sample for the laboratory experiments, but also by adopting a multi-level research design. The survey showed that the experimental results are in line with observation of attitudes towards a higher number of groups outside the lab. The cross-national analysis of surveys from 87 countries then tested one of the implications of the theory beyond India.

Laboratory experiments are also often criticized for not proving that the treatment and outcome variables correspond to a meaningful real-world phenomenon. Departing from the usual practice in laboratory research, I asked subjects not only how they understood the survey questions, but also what motivated their choices. Qualitative evidence provides more support for the tested theory. It also increases confidence in internal validity of the used indicators. Finally, interviews with subjects can identify problems with the research design, as the example of the detected house money effect demonstrated. Since experiments and interviews have complementary strengths, I believe that experimental research would benefit from implementing mixed methods more often.

6.4 Policy Implications

The experiment in the slums of Mumbai demonstrated that it is possible to change deep-rooted stereotypes and reduce discrimination between ethnic groups despite pre-existing tensions and a long history of ethnic conflict. The relatively simple experimental intervention had a considerable effect given the fact that I was working with real ethnic groups and strong prejudices.

Encouraging positive reciprocity between groups is a useful alternative to costly and unreliable interventions attempting to manipulate pre-existing identities: recategorization and decategorization. The decategorization approach emphasizes individual identity over social identity. For example, an advertisement may stress the occupational and not the racial identity of a political leader. Recategorization aims to recategorize two group identities as part of a common superordinate category. For example, the European Union exerts much effort to convince the French and Germans that they are not only two separate nations, but also members of a larger European community. However, even social psychologists themselves admit that their usual prescriptions of recategorization and decategorization of social identities are hardly applicable outside scientific laboratories (Brewer 1997).

Positive intergroup contact seems to work better (Pettigrew and Tropp 2006), but the “optimal conditions of positive contact” are impractically restrictive. This study suggests that we should focus on one particular aspect of intergroup contact, which is the positive reciprocity in indirect exchange. As the experiment in India showed, positive reciprocity improves relations even between groups that hold unequal social status, which

is the case in many hierarchical and post-conflict societies.

The demonstrated possibility of micro-level interventions increasing trust in strangers has interesting implications for the field of economic development. Previous studies have suggested that ethnic diversity does not necessarily lead to bad economic outcomes in democratic (Collier 2000) and developed (Alesina and La Ferrara 2005) countries. Nation-building emphasizing a common language can also prevent the negative effect of ethnic heterogeneity (Miguel 2004). However, it should be easier, faster, and much less costly to manipulate expectations of cooperative behavior than to build a nation with an inclusive common identity, well-functioning democratic institutions, and a high GDP per capita.

People update beliefs about other groups after they get to know their members. For intergroup contact to work, this study suggests that we do not need to satisfy all the "optimal conditions" specified by the contact hypothesis literature (Allport 1954; Pettigrew 1971; Pettigrew and Tropp 2006). Instead, we can use thousands of different ways to encourage intergroup cooperation and indirect reciprocity when intergroup contact occurs. Every successful cooperative interaction between individual group members will update their stereotypes a little bit and thus contribute to the overall improvement of intergroup relations in the long run.

Members of different groups can meet and cooperate, for example, if they belong to the same horizontal social network, such as those built by civil society organizations with inclusive membership (Putnam et al. 1993; Varshney 2002). Sometimes, exposure to cooperative members of another group can be also mandated from above. For example, Beaman et al. (2009) showed that randomly assigned gender quotas across Indian village

councils eliminate the negative bias in how male villagers perceive female leaders' effectiveness. Racial integration in military forces and schools is another example.

In addition to engaging in social exchange, for example by helping a member of a different ethnic group (Chapter 4), people can signal their cooperative type also by symbolic exchange. Malinowski (1926) observed more than 80 years ago that almost every ceremonial or religious act in primitive societies was regarded as a duty that one individual or social group owed to another individual or social group – not to a deity. By fulfilling our other social obligations, we signal that we are trustworthy enough for economic exchange as well. One of the recurrent themes in the interviews that I conducted in India was that the interviewed Hindus appreciated a lot if Muslims invited them to their homes or participated in a Hindu religious festival.

To what extent stereotypes about cooperativeness can be updated through dissemination of information instead of direct experience is an empirical question. There is some evidence that learning about acts of cooperation from written sources can reduce people’s underestimation of the extent of pro-social behavior. This is why many private fundraisers inform potential donors about other people’s contributions (Meier 2006). Future field experiments disseminating information about outgroups’ cooperativeness through the media or educational campaigns will hopefully measure the effect of mass communication on discrimination using the causal link of positive reciprocity.

A long strand of literature focuses on the effect of monitoring and punishment on cooperation. The number of defections usually drops whenever a third party has an ability to identify and punish defectors. Consequently, a higher cooperation rate in the present caused by credible enforcement should also increase expectations of future cooperation.

Chapter 4 suggested that stronger monitoring capacities within groups may explain why so many societies have ended up with ingroup bias. However, institutions with an authority over more than one group can expand the umbrella of credible enforcement to cover intergroup interactions as well. Herreros and Criado (2008, 55) argued that "the state can affect the citizen's expectation about other people's trustworthiness by acting as a third-party enforcer of agreements." Which specific state institutions matter? I expect institutions supporting the rule of law to be particularly important. As Rothstein and Stolle (2008, 445-6) explained:

> "Institutions of law and order have one particularly important task, to detect and punish people who break contracts and who therefore should not be trusted. Thus, if citizens think that these order institutions do what they are supposed to do in a fair, reasonably efficient, and unbiased manner, then they also have reason to believe that the chance people will get away with treacherous behavior is relatively small. They thus conclude that most people can be trusted."

Establishing expectations of generalized reciprocity, political institutions can contribute to better relations between groups in multiethnic countries. On the other hand, state failure or ethnic bias overtly displayed by the bureaucracy can erode interethnic trust. As the example of Tanzania analyzed in Miguel (2004) demonstrates, carefully designed state policies can help bridge ethnic differences. However, a strong state power can also undermine intergroup trust by purposeful destruction of horizontal networks – as Boix and Posner (1998) described in the case of South Italy.

In order to create generalized expectations of reciprocity, political institutions should be regarded as both effective and fair (Rothstein and Stolle 2008). People are

more likely to expect cooperation from outgroup members if they can trust the justice system to punish defectors. If the police is underfunded and corrupted, or if courts do not act in a timely fashion, the state ceases to be an effective arbiter of intergroup disputes. State institutions should be also believed to apply law indiscriminately on all citizens, regardless of their group membership. Effective but unfair institutions of law may persuade members of the politically dominant group that they live in a cooperative world, but they will not increase trust among oppressed groups.

In principle, reciprocity can be generalized to all groups even without state – by socialization. If a vast majority of people internalizes the social norms of reciprocity (obligation to help someone who has helped us) or general altruism (obligation to help others in general), their outgroup partners can expect cooperative behavior even without much enforcement. According to Dovidio (1984), social norms of reciprocity and general altruism can be sustained by affective feelings of fear, shame, or pride in addition to more material rewards (social status) and punishments (social sanctions).

Let me end by warning against dangers of political and economic inequality that can destroy positive reciprocity in the society. Alesina and La Ferrara (2002) found that people living in a racially mixed neighborhood with a high income disparity are much less trustful. A study focused on reciprocal behavior by Phan et al. (2009, 899) made a similar conclusion: “Neighborhood deprivation is found to exacerbate the exclusion of poorer individuals from informal helping; it also makes it harder for residents to reciprocate when they do participate in helping activities.” There are two major reasons to expect less cooperation in unequal societies. First, poor people have less resources to spend on others. As a result, their partners have less incentive to cooperate with them.

Second, inequality allows the powerful to use exploitation as a strategy alternative to reciprocity. Gouldner (1960) argued that reciprocity is less essential for people who have many alternative resources of the services that they receive from the other side. Moreover, members of a subordinate group may not seek punishment of occasional defections by members of the dominant group because they cannot afford jeopardizing the relationship they depend on. In other words, reciprocity can break down in highly unequal societies because the weak have nothing to contribute or they can be easily exploited. With social inequality rising in most countries of the world, positive reciprocity may become more difficult to achieve both in the slums of Mumbai and in developed countries.

Appendix A: Additional Tables and Figures

Table A.1: Descriptive Statistics (Full Sample)

Variable	Mean	SD	Min	Max	N
Distrust of Muslims	0.43	0.50	0	1	393
Social Distance (Neighbors)	0.59	0.49	0	1	402
Social Distance (Marriage)	0.73	0.45	0	1	402
Bhendi Bazar	0.37	0.48	0	1	401
Hindu	0.86	0.35	0	1	402
Age	0.00	9.80	-12.48	27.52	400
Education	0.00	3.71	-10.64	4.36	396
Income	0.00	31.43	-19.05	278.95	395
Extremist Voter	0.34	0.47	0	1	402
Salience of Marathi Identity	3.62	1.25	1	5	400
Salience of Hindu Identity	0.87	0.34	0	1	401
Quantity of Contact	0.17	1.04	-2	2	391
Quality of Contact	-0.15	1.18	-2	2	390
Status Differential	1.48	2.58	-6	9	383

Age, Education, and Income are centered at their means. Income is in thousands.

Table A.2: Logistic Regression Predicting Distrust of Muslims

	Generalized Reciprocity and Cooperative Hindus groups		Cooperative Muslims and No Reciprocity groups	
Model	(1)	(2)	(3)	(4)
Generalized Reciprocity	-1.049*	-1.294*		
	(0.466)	(0.577)		
Cooperative Muslims			-0.570	-0.726
			(0.421)	(0.491)
Magathane		-0.066		0.910
		(0.823)		(0.617)
Shivaji Nagar		-0.609		0.821
		(0.899)		(0.850)
Bhendi Bazar		-1.035.		0.171
		(0.609)		(0.559)
Hindu		-0.127		0.155
		(0.743)		(0.561)
Age		0.049		-0.041
		(0.032)		(0.032)
Education		-0.150*		0.149.
		(0.063)		(0.088)
Income		0.008		0.005
		(0.007)		(0.007)
Extremist Voter		0.736		0.620
		(0.661)		(0.535)
Intercept	-0.174	0.387	0.442	-0.599
N	90	84	93	85

Design-based robust standard errors in parentheses.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.3: Logistic Regression Predicting Social Distance

	Would not accept a Muslim neighbor		Would not accept intermarriage with a Muslim	
Model	(1)	(2)	(3)	(4)
Generalized	-1.682***	-1.706***	-1.447***	-1.734***
Reciprocity	(0.348)	(0.381)	(0.335)	(0.381)
Cooperative	-0.680*	-0.726*	-0.261	-0.165
Hindus	(0.331)	(0.348)	(0.383)	(0.389)
Cooperative	-1.265***	-1.238***	-1.159***	-1.075**
Muslims	(0.328)	(0.343)	(0.335)	(0.372)
No Reciprocity	-0.726*	-0.417	-0.457	-0.463
	(0.328)	(0.350)	(0.364)	(0.407)
Magathane		0.666*		-0.566.
		(0.287)		(0.323)
Shivaji Nagar		0.572.		0.250
		(0.299)		(0.335)
Bhendi Bazar		0.033		-0.388
		(0.230)		(0.267)
Hindu		0.152		0.683*
		(0.334)		(0.338)
Age		0.007		-0.013
		(0.012)		(0.013)
Education		0.041		-0.028
		(0.034)		(0.035)
Income		0.004		-0.000
		(0.004)		(0.004)
Extremist Voter		0.212		0.189
		(0.245)		(0.276)
Intercept	0.893***	0.256	1.447***	1.110**
N	402	386	402	386

Design-based robust standard errors in parentheses.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.4: Test of Heterogeneous Treatment Effects

	Would not accept a Muslim neighbor		Would not accept intermarriage with a Muslim	
Model	(1)	(2)	(3)	(4)
T1 (Generalized Reciprocity)	-0.262 (0.809)	-1.580*** (0.404)	-1.670. (0.870)	-1.066** (0.381)
T2 (Cooperative Hindus)	-1.738. (0.922)	-0.682. (0.390)	-1.670. (0.873)	-0.150 (0.434)
T3 (Cooperative Muslims)	-1.738. (0.919)	-0.969* (0.389)	-1.670. (0.871)	-0.884* (0.397)
T4 (No Reciprocity)	0.208 (0.841)	0.011 (0.450)	-1.224 (0.870)	0.082 (0.411)
Hindu	0.457 (0.479)		-1.222 (0.588)	
Extremist Voter		0.612. (0.331)		0.880* (0.411)
Hindu*T1	-1.745. (0.903)		0.274 (0.949)	
Hindu*T2	1.335 (0.996)		1.897. (0.999)	
Hindu*T3	0.595 (0.987)		0.629 (0.950)	
Hindu*T4	-1.099 (0.915)		0.981 (0.971)	
Extremist*T1		-0.120 (0.808)		-1.391. (0.827)
Extremist*T2		0.199 (0.769)		-0.197 (0.951)
Extremist*T3		-0.912 (0.735)		-0.880 (0.762)
Extremist*T4		-1.791* (0.692)		-1.440. (0.775)
Intercept	0.486	0.682***	1.477**	1.171***
N	402	402	402	402

Design-based robust standard errors in parentheses.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.5: Logistic Regression of Social Distance on Altruism

	Neighbors	Intermarriage
Generalized Reciprocity	-1.470 **	-1.355 **
	(0.487)	(0.502)
Cooperative Hindus	-0.321	0.351
	(0.468)	(0.519)
Cooperative Muslims	-0.802 .	-0.639
	(0.445)	(0.493)
Altruism	0.092	-0.116
	(0.151)	(0.176)
Magathane	0.829 .	0.580
	(0.497)	(0.547)
Shivaji Nagar	0.850	0.313
	(0.567)	(0.560)
Bhendi Bazar	-0.456	-0.764 *
	(0.342)	(0.364)
Hindu	-0.086	0.687
	(0.448)	(0.460)
Age	0.022	0.004
	(0.021)	(0.022)
Education	0.091 *	-0.032
	(0.045)	(0.038)
Income	0.004	0.002
	(0.005)	(0.005)
Extremist Voter	-0.277	-0.148
	(0.377)	(0.396)
Intercept	0.123	0.730
N	177	177

Design-based robust standard errors in parentheses. The No Reciprocity group is a baseline.

. $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6: Linear Regression Predicting Altruism Change Score

Model	(1)	(2)
Generalized Reciprocity	-0.104	-0.097
	(0.185)	(0.201)
Cooperative Hindus	-0.172	-0.128
	(0.240)	(0.253)
Cooperative Muslims	0.185	0.125
	(0.203)	(0.208)
Magathane		0.153
		(0.190)
Shivaji Nagar		0.370
		(0.282)
Bhendi Bazar		0.178
		(0.193)
Hindu		-0.005
		(0.234)
Age		-0.012
		(0.013)
Education		0.028
		(0.023)
Income		-0.001
		(0.002)
Extremist Voter		0.177
		(0.164)
Intercept	-0.083	-0.401 .
N	192	177

Design-based robust standard errors in parentheses. The No Reciprocity group is a baseline.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.7: Logistic Regression of Social Distance on Social Identity Salience

	Would not accept a Muslim neighbor		Would not accept intermarriage with a Muslim	
Model	(1)	(2)	(3)	(4)
Generalized	-1.466***	-2.082***	-1.700***	-1.672***
Reciprocity	(0.395)	(0.441)	(0.399)	(0.427)
Cooperative	-0.472	-0.641	-0.094	0.209
Hindus	(0.375)	(0.394)	(0.410)	(0.491)
Cooperative	-0.993**	-1.236**	-1.037**	-0.970*
Muslims	(0.363)	(0.386)	(0.390)	(0.425)
No Reciprocity	-0.263	-0.704.	-0.462	-0.296
	(0.356)	(0.377)	(0.415)	(0.467)
Salience of	-0.211*		-0.027	
Marathi Id.	(0.105)		(0.104)	
Salience of		0.707.		0.303
Hindu Id.		(0.395)		(0.406)
Magathane	0.631*	0.713*	-0.545.	-0.654.
	(0.290)	(0.311)	(0.320)	(0.348)
Shivaji Nagar	0.544.	0.716*	0.267	0.367
	(0.301)	(0.330)	(0.332)	(0.385)
Bhendi Bazar	0.020	0.179	-0.367	-0.293
	(0.233)	(0.258)	(0.268)	(0.298)
Hindu	0.126		0.580.	
	(0.343)		(0.346)	
Age	0.004	0.007	-0.014	-0.015
	(0.012)	(0.012)	(0.013)	(0.015)
Education	0.046	0.028	-0.029	-0.024
	(0.035)	(0.036)	(0.036)	(0.040)
Income	0.008.	0.008	0.004	-0.000
	(0.005)	(0.005)	(0.006)	(0.005)
Extremist Voter	0.193	0.014	0.182	0.092
	(0.246)	(0.255)	(0.275)	(0.286)
Intercept	0.995*	-0.198	1.283*	1.442**
N	384	384	333	333

Design-based robust standard errors in parentheses.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.8: Logistic Regression Predicting Ingroup Favoritism in the Dictator Game

Model	(1)	(2)
Salience of Marathi Identity	-0.023	
	(0.102)	
Salience of Hindu Identity		0.108
		(0.237)
Magathane	0.963 ***	0.882 **
	(0.250)	(0.278)
Shivaji Nagar	0.720 **	0.475 .
	(0.241)	(0.250)
Bhendi Bazar	0.413 .	0.509 .
	(0.238)	(0.259)
Hindu	-0.327 .	
	(0.175)	
Age	-0.012	-0.014
	(0.010)	(0.011)
Education	-0.012	-0.017
	(0.020)	(0.021)
Income	-0.002	-0.003 .
	(0.001)	(0.002)
Extremist Voter	0.019	-0.058
	(0.216)	(0.249)
Intercept	1.375 *	0.965 **
N	175	144

Design-based robust standard errors in parentheses. The No Reciprocity group is a baseline.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.9: Quantity and Quality of Previous Intergroup Contact

	Would not accept a Muslim neighbor		Would not accept intermarriage	
Model	(1)	(2)	(3)	(4)
T1 (Generalized Reciprocity)	-1.765 ***	-1.682 ***	-1.457 ***	-1.450 ***
	(0.372)	(0.363)	(0.357)	(0.350)
T2 (Cooperative Hindus)	-0.604 .	-0.566	-0.146	-0.431
	(0.342)	(0.383)	(0.399)	(0.434)
T3 (Cooperative Muslims)	-1.210 ***	-0.969 **	-1.053 **	-0.986 *
	(0.361)	(0.371)	(0.361)	(0.389)
T4 (No Reciprocity)	-0.763 *	-0.759 *	-0.117	-0.349
	(0.341)	(0.352)	(0.424)	(0.389)
Quantity of Contact	0.009		0.079	
	(0.172)		(0.178)	
Quality of Contact		-0.100		0.084
		(0.151)		(0.150)
Quantity*T1	0.033		0.225	
	(0.337)		(0.320)	
Quantity*T2	0.217		-0.165	
	(0.313)		(0.319)	
Quantity*T3	-0.743		-0.782 *	
	(0.394)		(0.386)	
Quantity*T4	-0.079		-0.680	
	(0.335)		(0.422)	
Quality*T1		0.329		-0.117
		(0.298)		(0.299)
Quality*T2		0.289		-1.137 *
		(0.305)		(0.480)
Quality*T3		0.284		0.044
		(0.282)		(0.283)
Quality*T4		-0.233		-0.273
		(0.319)		(0.324)
Intercept	0.890 ***	0.907 ***	1.422 ***	1.440 ***
N	391	390	391	390

Design-based robust standard errors in parentheses.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.10: Logistic Regression of Social Distance on Relative Social Status

	Would not accept a Muslim neighbor	Would not accept intermarriage
T1 (Generalized Reciprocity)	-1.885 ***	-1.572 ***
	(0.102)	(0.390)
T2 (Cooperative Hindus)	-0.918 *	-0.210
	(0.412)	(0.423)
T3 (Cooperative Muslims)	-1.679 ***	-1.453 ***
	(0.420)	(0.415)
T4 (No Reciprocity)	-1.081 **	-0.466
	(0.391)	(0.417)
Status Differential	0.126 .	0.032
	(0.075)	(0.083)
Status*T1	0.035	0.187
	(0.136)	(0.140)
Status*T2	0.126	0.039
	(0.169)	(0.147)
Status*T3	0.103	0.243 .
	(0.129)	(0.142)
Status*T4	0.090	0.034
	(0.134)	(0.128)
Intercept	0.898 ***	1.333 ***
N	383	383

Design-based robust standard errors in parentheses.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.11: Descriptive Statistics for the Control Group

Variable	Mean	SD	Min	Max	N
Discrimination	0.70	0.36	0	1	840
Positive Reciprocity	0.37	0.48	0	1	840
Intergroup Contact	0.16	1.05	-2	2	840
Negative Reciprocity	-0.15	1.13	-2	2	840
Economic Threat	-0.12	1.10	-2	2	838
Safety Threat	-0.24	1.10	-2	2	840
Relative Group Size	0.10	0.09	0.005	0.19	840
Relative Group Status	-1.95	2.49	-9	8	780
Shared Hindu Identity	0.50	0.50	0	1	840
Identity Salience	3.09	1.11	1	5	840
Education	0.31	2.40	-6.64	4.36	840
Age	1.73	9.89	-12.48	27.52	840
Hindu	0.90	0.30	0	1	840

Table A.12: Ordinal Logistic Regression Predicting Perceived Threat

	Economic Threat	Safety Threat
Relative Group Size	8.297 ***	9.072 ***
	(0.887)	(0.917)
Intergroup Contact	0.325 *	0.282 *
	(0.138)	(0.134)
Positive Reciprocity	-0.009	0.086
	(0.208)	(0.210)
Negative Reciprocity	0.450 **	0.837 ***
	(0.140)	(0.147)
Shared Hindu Identity	-0.207	-0.848 ***
	(0.143)	(0.147)
Relative Group Status	0.223 ***	0.144 *
	(0.059)	(0.059)
Intercept Y ≥ -1	3.243 **	4.371 ***
Intercept Y ≥ 0	0.267	1.484
Intercept Y ≥ 1	-1.739	-0.506
Intercept Y ≥ 2	-4.547 ***	-3.614 **
N	778	780

Individual-level fixed effects. Robust standard errors in parentheses.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table A.13: Number of Respondents per Survey

Territory	1981-1984	1989-1993	1994-1999	1999-2004	2005-2007
Albania	0	0	999	1000	0
Algeria	0	0	0	1282	0
Andorra	0	0	0	0	1003
Argentina	1005	1002	1079	1280	1002
Armenia	0	0	2000	0	0
Australia	1228	0	2048	0	1421
Azerbaijan	0	0	2002	0	0
Bangladesh	0	0	1525	1500	0
Belarus	0	1015	2092	0	0
Bosnia and Herzegovina	0	0	1200	1200	0
Brazil	0	1782	1149	0	1500
Bulgaria	0	0	1072	0	1001
Burkina Faso	0	0	0	0	1534
Canada	0	0	0	1931	2164
Chile	0	1500	1000	1200	1000
China	0	1000	1500	1000	2015
Colombia	0	0	6025	0	3025
Croatia	0	0	1196	0	0
Cyprus	0	0	0	0	1050
Czech Republic	0	924	1147	0	0
Dominican Republic	0	0	417	0	0
Egypt	0	0	0	3000	3051
El Salvador	0	0	1254	0	0
Estonia	0	0	1021	0	0
Ethiopia	0	0	0	0	1500
Finland	1003	0	987	0	1014
France	0	0	0	0	1001
Georgia	0	0	2008	0	1500
Germany	0	0	2026	0	2064
Ghana	0	0	0	0	1534
Great Britain	0	0	1093	0	1041
Guatemala	0	0	0	0	1000
Hong Kong	0	0	0	0	1252
Hungary	1464	0	650	0	0
India	0	2500	2040	2002	2001
Indonesia	0	0	0	1004	2015
Iran	0	0	0	2532	2667

Iraq	0	0	0	2325	2701
Israel	0	0	0	1199	0
Italy	0	0	0	0	1012
Japan	1204	1011	1054	1362	1096
Jordan	0	0	0	1223	1200
Kyrgyzstan	0	0	0	1043	0
Latvia	0	0	1200	0	0
Lithuania	0	0	1009	0	0
Macedonia	0	0	995	1055	0
Malaysia	0	0	0	0	1201
Mali	0	0	0	0	1534
Mexico	1837	1531	2364	1535	1560
Moldova	0	0	984	1008	1046
Morocco	0	0	0	2264	1200
Netherlands	0	0	0	0	1050
New zealand	0	0	1201	0	954
Nigeria	0	1001	1996	2022	0
Norway	0	0	1127	0	1025
Pakistan	0	0	733	2000	0
Peru	0	0	1211	1501	1500
Philippines	0	0	1200	1200	0
Poland	0	938	1153	0	1000
Puerto Rico	0	0	1164	720	0
Romania	0	0	1239	0	1776
Russian Federation	0	1961	2040	0	2033
Rwanda	0	0	0	0	1507
Saudi Arabia	0	0	0	1502	0
Serbia	0	0	0	0	1220
Serbia and Montenegro	0	0	1520	2260	0
Singapore	0	0	0	1512	0
Slovakia	0	466	1095	0	0
Slovenia	0	0	1007	0	1037
South Africa	1596	2736	2935	3000	2988
South Korea	970	1251	1249	1200	1200
Spain	0	1510	1211	1209	1200
Sweden	0	0	1009	1015	1003
Switzerland	0	1400	1212	0	1241
Taiwan	0	0	780	0	1227
Tanzania	0	0	0	1171	0

Thailand	0	0	0	0	1534
Trinidad and Tobago	0	0	0	0	1002
Turkey	0	1030	1907	3401	1346
Uganda	0	0	0	1002	0
Ukraine	0	0	2811	0	1000
United States	0	0	1542	1200	1249
Uruguay	0	0	1000	0	1000
Venezuela	0	0	1200	1200	0
Vietnam	0	0	0	1000	1495
Zambia	0	0	0	0	1500
Zimbabwe	0	0	0	1002	0

Table A.14: Descriptive Statistics of the World Values Survey Data

Variable	Mean	SD	Min	Max	N
Public Goods	7.59	2.04	0	9	218588
Ethnic Fractionalization	0.40	0.23	0	0.93	254493
Trust	0.27	0.44	0	1	246798
Acceptance of Bribe	1.77	1.79	1	10	243824
Male	0.48	0.50	0	1	252941
Age	40.31	15.91	14	99	247978
Education	4.41	2.33	1	8	230283
Income	4.51	2.39	1	10	226003
Married	0.58	0.49	0	1	253001
Church Attendance	3.62	2.58	0	7	238981
Confidence in Government	2.51	0.68	0.35	5.03	248053
Tax Revenue	15.47	6.46	0.23	49.1	176388
Democracy	5.18	5.67	-10	10	233881

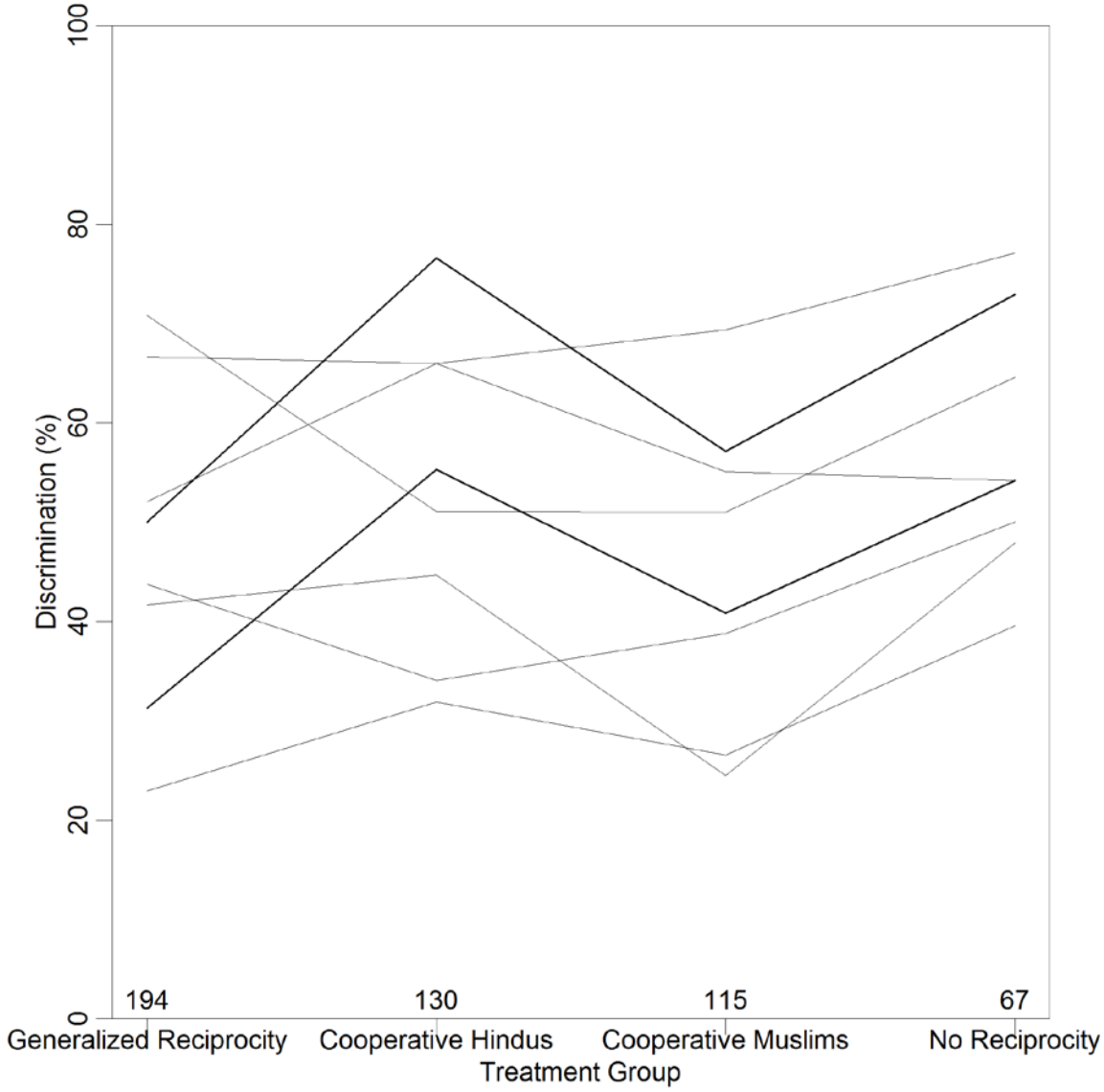
Table A.15: Multilevel Model of Willingness to Contribute to Public Goods

Ethnic Frac.	-0.342 **
	(0.119)
Trust	-0.069 **
	(0.024)
E. F. * Trust	0.090 .
	(0.051)
Acceptance of Bribe	-0.509 ***
	(0.003)
Male	-0.087 ***
	(0.010)
Age	0.010 ***
	(0.000)
Education	0.015 ***
	(0.003)
Income	-0.006 *
	(0.003)
Married	0.104 ***
	(0.011)
Church Attendance	0.032 ***
	(0.002)
Confidence in Government	0.143 ***
	(0.009)
Tax Revenue	0.008
	(0.004)
Democracy	-0.002
	(0.004)
Intercept	7.573 ***
N (individuals)	103848
N (regions)	841

Standard errors in parentheses.

. p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Figure A.1: Discrimination against Muslims (Thick Lines) and Other Outgroups (Thin Lines)



Appendix B: Questionnaire Used in India

q1: “Can you read and write in Marathi?”

Answers: 1. Yes. / 0. No.

q2: “Your access to water is from:”

Answers: 1. Individual tap. / 2. Shared tap. / 3. Stand post. / 4. No access to water where you live.

q3: “You usually use:”

Answers: 1. An individual toilet. / 2. A common toilet in your building. / 3. A pay to use toilet. / 4. A free public toilet. / 5. No toilet.

q4: “Was your garbage collected at least once in the last 7 days?”

Answers: 1. Yes. / 0. No. / 999. Don't know.

“In the past six months, have community members from your neighborhood organized community efforts to...”

q5: ...distribute water?

q6: ...build, maintain or clean public toilets?

q7: ...organize garbage collection for this area?

Answers: 1. Yes. / 0. No. / 999. Don't know.

q8: “Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?”

Answers: 1. Most people can be trusted. / 0. Need to be very careful.

“I would like to ask you a question about how much trust you have in people from various groups. For each, please tell me whether you have a lot of trust, some trust, not very much trust or no trust at all?”

q9: Maharashtrians

q10: Muslims.

q11: Gujaratis.

q12: Biharis.

q13: Parsis.

Answers: -2. No trust at all. / -1. Not very much trust. / 1. Some trust. / 2. A lot of trust.

“In the past twelve months, which people, if any, have helped you directly by giving you money or some of their time?”

Answers: Answers: 1. Yes. / 0. No.

q14: Maharashtrians

q15: Muslims

q16: Gujaratis

q17: Biharis

q18: Parsis

“In the past twelve months, for which people, if any, have you helped directly by giving some of your money or time?”

Answers: Answers: 1. Yes. / 0. No.

q19: Maharashtrians.

q20: Muslims.

q21: Gujaratis.

q22: Biharis.

q23: Parsis.

“How often do you have informal talks with...?”

Answers: -2. Never / -1. Seldom. / 0. Occasionally. / 1. Often. / 2. Very often.

q24: Maharashtrians.

q25: Muslims.

q26: Gujaratis.

q27: Biharis.

q28: Parsis.

“How often have you been taken advantage of by... ?”

Answers: -2. Never / -1. Seldom. / 0. Occasionally. / 1. Often. / 2. Very often.

q29: a Maharashtrian.

q30: a Muslim.

q31: a Gujarati.

q32: a Bihari.

q33: a Parsi.

“How often have you been treated as inferior by...?”

Answers: -2. Never / -1. Seldom. / 0. Occasionally. / 1. Often. / 2. Very often.

q34: a Maharashtrian.

q35: a Muslim.

q36: a Gujarati.

q37: a Bihari.

q38: a Parsi.

q39: If it was up to you, would you accept a Maharashtrian to close kinship by marriage?

Answers: Answers: 0. Yes. / 1. No.

q40: If it was up to you, would you accept a Muslim to close kinship by marriage?

Answers: Answers: 0. Yes. / 1. No.

q41: If it was up to you, would you accept a Gujarati to close kinship by marriage?

Answers: Answers: 0. Yes. / 1. No.

q42: If it was up to you, would you accept a Bihari to close kinship by marriage?

Answers: Answers: 0. Yes. / 1. No.

q43: If it was up to you, would you accept a Parsi to close kinship by marriage?

Answers: Answers: 0. Yes. / 1. No.

q44: If it was up to you, would you accept a Maharashtrian as a neighbor?

Answers: Answers: 0. Yes. / 1. No.

q45: If it was up to you, would you accept a Muslim as a neighbor?

Answers: Answers: 0. Yes. / 1. No.

q46: If it was up to you, would you accept a Gujarati as a neighbor?

Answers: Answers: 0. Yes. / 1. No.

q47: If it was up to you, would you accept a Bihari as a neighbor?

Answers: Answers: 0. Yes. / 1. No.

q48: If it was up to you, would you accept a Parsi as a neighbor?

Answers: Answers: 0. Yes. / 1. No.

q49: “How often do you lend personal possessions (for example bicycle, mobile phone, clothes, etc.) to your relatives, friends, and other people?”

Answers: 2. Very often. / 1. Often. / 0. Sometime. / -1. Rarely. / -2. Never.

q50: “Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?”

Answers: 0. Would take advantage. / 1. Would try to be fair.

q51. “How much confidence do you have in the police?”

Answers: -2. No confidence at all. / -1. Not very much confidence. / 0. Some confidence. / 1. Quite a lot of confidence. / 2. A great deal of confidence.

q52: “How successful do you think the government in India is nowadays in controlling crime?”

Answers: -2. Very unsuccessful. / -1. Quite unsuccessful. / 0. Neither unsuccessful, nor successful. / 1. Quite successful. / 2. Very successful.

q53: “In your opinion, how often do elected public officials and civil servants deal fairly with people like you?”

Answers: 2. Almost always. / 1. Often. / 0. Occasionally. / -1. Seldom. / -2. Almost never.

q54: “Please tell me whether you think avoiding a fare on public transport can always be justified, never be justified, or something in between.”

Answers: 1. Always justified. / 2. Often justified. / 3. Occasionally justified. / 4. Seldom justified. / 5. Never justified.

q55: “Think of this ladder as representing where people stand in India. At the top of the ladder are the people who are the best off, those who have the most money, most education, and the most respected jobs. At the bottom are the people who are the worst off, those who have the least money, least education, and worst jobs or no job. The higher up you are on this ladder, the closer you are to the people at the very top. The lower you are, the closer to the people at the very bottom you are. Where would you place the Maharashtrians on this ladder, relative to other people in India?”

Answers: 1 / 2 / 3 / 4 / 5 / 6 / 7 / 8 / 9 / 10

q56: „Where would you place Muslims?“

Answers: 1 / 2 / 3 / 4 / 5 / 6 / 7 / 8 / 9 / 10

q57: „Where would you place Gujaratis?“

Answers: 1 / 2 / 3 / 4 / 5 / 6 / 7 / 8 / 9 / 10

q58: „Where would you place Biharis?“

Answers: 1 / 2 / 3 / 4 / 5 / 6 / 7 / 8 / 9 / 10

q59: „Where would you place Parsis?“

Answers: 1 / 2 / 3 / 4 / 5 / 6 / 7 / 8 / 9 / 10

Question: “How much do you agree or disagree with the following statements?”

Answers: -2. Disagree strongly. / -1. Disagree. / 0. Neither agree, nor disagree. / 1. Agree. / 2. Agree strongly.

q60. Maharashtrians take jobs away from people like me.

q61. Muslims take jobs away from people like me.

q62. Gujaratis take jobs away from people like me.

q63. Biharis take jobs away from people like me.

q64. Parsis take jobs away from people like me.

“How much do you agree or disagree with the following statements?”

Answers: -2. Disagree strongly. / -1. Disagree. / 0. Neither agree, nor disagree. / 1. Agree. / 2. Agree strongly.

q65. Maharashtrians increase crime rates.

q66. Muslims increase crime rates.

q67. Gujaratis increase crime rates.

q68. Biharis increase crime rates.

q69. Parsis increase crime rates.

q70: “We are all parts of different groups. People may feel different degrees of attachment to their groups. Please tell me how attached you feel to India.”

Answers: 5. Very attached. / 4. Fairly attached. / 3. Somewhere in between. / 2. Not very attached. / 1. Not at all attached.

q71: “Please tell me how attached you feel to Maharashtra.”

Answers: 5. Very attached. / 4. Fairly attached. / 3. Somewhere in between. / 2. Not very attached. / 1. Not at all attached.

q72: “How often do you pray?”

Answers: 1. Daily. / 2. Weekly. / 3. Only on festivals. / 4. Never.

q73: “In what year were you born?”

Answers: [number]

q74: “In which state and district were you born?”

Answers: [text]

q76: “In what year did you come to this neighborhood?”

Answers: [number]

q77: “What is your religion?”

1. Hindu.

2. Muslim.

3. Christian.

4. Buddhist.

5. Other or no religion.

q78: “What is your Jati / caste / tribe name?”

Answers: [text]

q79: “What language do you normally speak at home?”

Answers: Marathi. / Hindi. / Gujarati. / Other.

q80: “How many years of schooling have you had?”

Answers: [number]

q81: “What was the total income of your household last month, putting together the income of all members of the household? Write in the number of Rupees.”

Answers: [number]

q82: “Now I will ask you about the last Lok Sabha elections held in 2009? I mean the MP elections for electing the Central Government in Delhi. Which political party did you vote for?”

1. Bahujan Samaj Party (BSP).
2. Bahujan Vikas Aghadi (BVA).
3. Bharatiya Janata Party (BJP).
4. Indian National Congress (INC).
5. Maharashtra Navnirman Sena (MNS).
6. Nationalist Congress Party (NCP).
7. Samajwadi Party (SP).

8. Shivsena (SHS).

9. Swabhimani Paksha (SWP).

10. Other political party or independent candidate.

11. Did not vote.

12. Does not remember.

q83: “Do you have any acquaintances in Dadar?”

Answers: 1. Yes. / 0. No.

q84: “Do you have any acquaintances in Bhendi Bazar?”

Answers: 1. Yes. / 0. No.

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